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Walter LaFeber — Kissinger and Acheson: The Secretary of State and the Cold War

Andrew M. Greeley — How Conservative are American Catholics?

Richard P. Nathan, Paul R. Dommel, Sarah F. Liebschutz, Milton D. Morris — Monitoring the Block Grant Program for Community Development

Philip W. Dyer — Tactical Nuclear Weapons and Deterrence in Europe

Lewis Brownstein — Decision Making in Israeli Foreign Policy: An Unplanned Process

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Political Science Quarterly

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How real the Soviet threat is to the United States is one of the most perplexing questions facing Americans today. The Academy of Political Science, at its fall conference to be held at Columbia University in New York City, will consider "The Soviet Threat: Myth or Reality?" Detailed programs will be mailed to Academy members.

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From the Managing Editor:

In this issue . . .

WALTER LAFEBER reviews recent biographies of secretaries of state Dean Acheson and Henry Kissinger, noting that the two men were strikingly similar in their views of Europe, the southern hemispheres, and the American public. According to Lafeber, Acheson and Kissinger represented a break in the tradition of secretaries who had strong credentials in domestic politics, a tradition that now needs to be revived.

ANDREW M. GREELEY analyzes the political attitudes and voting behavior of American Catholic ethnics. He concludes that the popular view of Catholic ethnics being politically conservative or reactionary is not substantiated by empirical evidence.

RICHARD P. NATHAN, PAUL R. DOMMEL, SARAH F. LIEBSCHUTZ, and MILTON D. MORRIS report on the impact of the block grant program authorized by the Housing and Community Development Act of 1974. They find that under the new program, "generalist" local officials have gained a more prominent role in deciding how the grant money is spent, citizen participation in the decision-making process has increased, and there has been a focus on capital spending. The authors also find that the distribution formula has favored small and suburban jurisdictions while disadvantaging older and more distressed central cities.

PHILIP W. DYER argues that the NATO tactical nuclear weapons presently positioned in Europe not only do not provide enough deterrent value to justify their existence, but that they complicate NATO planning more than that of the Soviets and should, therefore, be removed.

LEWIS BROWNSTEIN examines how foreign policy has been made in Israel in recent years. He shows that the process has been dominated by a small elite and that there has been lack of advance planning, an underdevelopment of in-house research capability, and a resistance to the use of outside experts.

LARRY BERMAN discusses the efforts of three presidentially appointed advisory groups in trying to improve management practices within the executive branch. He contends that in all these efforts, the Bureau of the Budget obstructed proposed changes in the Executive Office of the President because it wished to preserve its post-1939 status as first among equals.

DEMETRIOS CARALEY

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Kissinger and Acheson: The Secretary of State and the Cold War

WALTER LAFEBER

Since World War II, the secretary of state's central concerns, if diagramed, resemble a wishbone. Projecting from a common center, one side extends toward areas which before 1945 were of little concern to the United States: the eastern Mediterranean, Korea, Indochina, and now Africa. The other side points toward the secretary of state's need to create and maintain a public consensus at home to support these new policies. The latter task has not been easy during many of the postwar years. Support has been hard to retain for involvements in out-of-the-way corners of the globe; or, as in the case of Indochina, it has been difficult to hide that involvement from the public and then, when it is discovered, to explain it away. The obligations overseas and those at home have consequently often been at cross-purposes. The tension has grown during the 1970s until the wishbone has nearly snapped, forcing the secretary of state into quick shuttle diplomacy, vague agreements that promise more than they can deliver, and subtle manipulation of the media. Such devices, however, have only temporarily relieved the tension.

If Dean Acheson was "present at the creation" of the secretary of state's bifurcated responsibility, Henry Kissinger was present when Acheson's creation nearly came apart. Acheson did not act alone in creating the postwar global order, and Kissinger, contrary to the carefully cultivated "lone cowboy" image, did not single-handedly attempt to hold up that sagging order. But each man brought a world view and a personality to his task that were of historical importance. David McClellan's lengthy biography, *Dean Acheson*, Bruce Mazlish's

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WALTER LAFEBER is professor of history at Cornell University. He is the author of *The New Empire: America, Russia and the Cold War*, whose third edition just appeared; and a forthcoming work on the United States and Panama.

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Kissinger, and John G. Stoessinger's *Henry Kissinger* explore the opening and closing of an era in world history.[1]

These volumes raise two particularly disturbing questions: first, whether even an intelligent and powerful secretary of state's conceptualization of world order (a conceptualization whose foresight and coherence enable him to guide and dominate others), is necessarily so limited, perhaps anachronistic, in regard to non-Western areas that his diplomacy is inadequate from the start; and second, whether in order to keep the wishbone connected a secretary of state must manipulate American society rather than keep it accurately informed.

The primary importance of Acheson and Kissinger is their attempt to translate personal world views into a new global order.[2] McClellan does not coherently explore Acheson's conceptual framework, although the secretary personally considered such a framework of central importance. Arguing in 1954 that "Governmental policy is an integer," and that diplomatic, military, economic, and fiscal policy is "all one," Acheson asked: "Do we cut the pattern according to the cloth or do we cut the cloth according to the pattern?" He answered, "The first requirement is that there must be a pattern."[3] Six years later, Acheson approvingly quoted Roger Hilsman: "It is almost traditional in America to view foreign affairs as a problem in public administration," but "few of our true failures are attributable to bad administration in carrying policy out. Our true failures probably lie more often in failing to recognize emerging problems in time."[4] Although he placed a high priority on a smoothly running department, Acheson apparently did not share the view of many scholars who believed American foreign policy suffered more from bureaucratic inefficiency than from a dearth of good ideas. McClellan notes, for example, that Acheson neglected the Policy Planning Staff not for organizational reasons, but because differences between the secretary of state and George Kennan, the staff's director, were "also philosophic and substantive."[5]

Acheson's most important concept, McClellan writes, "involved the twofold process of exploiting the [Soviet] threat in order to overcome the threat and to reconstitute the chaotic and demoralized Western state system."[6] His passion for order at least equaled Kissinger's. McClellan argues that Acheson, contrary to many in his "social class," had "a remarkable ability to accept social change

[1] David S. McLellan, *Dean Acheson: The State Department Years* (New York, 1976); Bruce Mazlish, *Kissinger: The European Mind in American Diplomacy* (New York, 1976); John G. Stoessinger, *Henry Kissinger: The Anguish of Power* (New York, 1976).

[2] On the absence of conceptual planning in American foreign policy, a provocative essay is Zbigniew Brzezinski, "Purpose and Planning in Foreign Policy," *Public Interest*, XIV (Winter 1969), 52–73.

[3] Dean Acheson, "The Responsibility for Decision in Foreign Policy," *Yale Review*, XLIV (Autumn 1954), 1–12.

[4] Don K. Price (ed.), *The Secretary of State* (Englewood Cliffs, N. J., 1960), pp. 36, 47–48.

[5] McLellan, *Acheson*, p. 172.

[6] Ibid., p. 380.

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with equanimity." The evidence in the book indicates otherwise, for Acheson quit the New Deal in 1933 because of Roosevelt's desire for rapid change, and as secretary of state he refused to recognize the need for radical reform in Asia and Latin America. His reluctance to work for social change was particularly remarkable since he had served as a law clerk with Louis Brandeis (in whose office, McClellan writes, "Acheson became acutely aware of many of the brutal social conditions of the era").[7] His quest for global order led him into areas new to American diplomacy, in order to oppose revolutionary movements in those and other areas, and, in such vital places as Greece and China, to confuse national movements with Russian expansionism.

Kissinger's policies also emerged from a world view that assumed the need for order. As might be expected, Mazlish sometimes uses psychohistory to explain that need, but his major contribution is in utilizing a wide knowledge of nineteenth- and twentieth-century Europe to write an intellectual history that judges Kissinger on his own terms, and pronounces him a failure. For although Kissinger wanted to temper American innocence and optimism with a European sense of tragedy, "he never succeeded in fusing the two traditions."[8]

In tracing Kissinger's important distinction between a desirable "legitimate" and an undesirable "revolutionary" state, Stoessinger recalls a conversation when he and Kissinger were graduate students at Harvard:

> What, I asked, would be his view if the leader of a legitimate state pursued unjust ends while the revolutionary had justice on his side? Then, the statesman must choose, Kissinger replied without hesitation. How, I wondered. Kissinger replied with a quotation from Goethe: "If I had to choose between justice and disorder, on the one hand," he said, "and injustice and order, on the other, I would always choose the latter."[9]

The most striking aspect of such an idea is not that its roots are in a particular nineteenth-century European order, or that it emanated out of a rebellion against the disorder that created Nazi and Communist power. Most striking is the similarity of Kissinger's world view to that of Acheson. The Jewish emigré and the son of an Episcopalian minister from Connecticut came to see the globe in much the same way. The two statesmen differed from the rest of their countrymen only in being able to make that view more coherent, historically rooted, and politically effective.

The differences between the two men are so minor as to be enlightening. Kissinger wrote extensively about Metternich and Bismarck, but his hero was England's Castlereagh. Acheson picked up the story at that point. During the 1930s, McClellan observes, Acheson "acquired a great admiration . . . for the Victorian period" of British history, especially London's ability to use its capital as a cornerstone of "world peace and stability. The role that Britain had played would

[7] Ibid., pp. 14–17.
[8] Mazlish, *Kissinger*, pp. 297–298.
[9] Stoessinger, *Henry Kissinger*, pp. 12–14.

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be at the forefront of Acheson's mind in 1945 when America faced the choice between exercising a constructive role in the world or returning to isolationism."[10]

Despite similar grounding in nineteenth-century Europe, however (and here the irony is particularly revealing), both men shared with many Americans a mistrust of Western European initiatives. Acheson's determination to create the Marshall Plan and NATO emerged from fundamental American, not European, needs, and McClellan offers an interesting thesis that Acheson was distinctly cool toward European integration, particularly the European Defense Community. The main American objective was German rearmament, and Acheson tolerated unification policies in order to obtain the rearmament. Stoessinger is more blunt about Kissinger: "Europe brought out the darker side of his personal diplomacy." His "Year of Europe" speech "reminded one European diplomat of an unfaithful husband's decision to declare a 'year of the wife,' " nor did it help when he blurted out that he "didn't care what happened to NATO," and delivered lectures to Portugal that deeply angered Western Europeans.[11] Such thoughts from two secretaries of state who were supposedly "European" in viewpoint makes more understandable the reciprocal mistrust between Dulles and Kennedy, on the one hand, and France and Great Britain, on the other. One begins to wonder whether the Atlantic community's interests are as common as the Trilateral Commission and other Atlanticists believe.

Thus although both men dealt with European problems, their most important legacy might well become their policies toward non-Western, revolutionary areas. Acheson and Kissinger are sometimes considered "conservative," but the term is useful only if it means they were antirevolutionary. Neither man possessed the sense of proportion and limit associated with classical conservatives. As Mazlish notes, in both his senior thesis and later interviews, Kissinger urged "an active recognition of limits," but the Cambodian operations of 1969–1971, the Christmas bombing of Vietnam, and the wiretapping of his associates did not demonstrate that Kissinger acted on his own advice. Similarly, Acheson placed more confidence in American power than in the wisdom of recognizing national limits when he shaped the rhetoric of the Truman Doctrine and allowed MacArthur to go to the Yalu.

The two secretaries did part company over economic policy. McClellan summarizes its importance to Acheson. To "revisionists," he writes,

> Acheson was nothing more than a mouthpiece for big business. To be sure, a capitalist economy functions best in the context of a multilateral trading world, and the American economy, being the most powerful, could benefit disproportionately from such a context. But, paradoxically, Acheson's arguments were an attempt to convince the representatives of the capitalist system . . . that America had a stake in restoring the conditions of a multilateral trading world.[12]

[10] McLellan, *Acheson*, pp. 30–31.
[11] Stoessinger, *Henry Kissinger*, p. 219.
[12] McLellan, *Acheson*, p. 93.

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(The book is marred by McClellan's beside-the-point comments about revisionist history. He apparently did not realize that in this instance, among others, most revisionists would not disagree with his own assessment of Acheson.)

If Acheson's policies rested on a comprehensive analysis of capitalist needs, including an orderly world in which business leaders could project long-term plans, Kissinger's world view was fundamentally, perhaps fatally, flawed by a neglect of economics. In this he only reflected the denigration and ignorance of economic factors found in many of the international relations works and "consensus" histories that misshaped American intellectual life during the 1950s and early 1960s. Mazlish observes that Kissinger's assumption "that the existence and expansion of Hitler's power was abetted by foolish men of goodwill" who knew nothing of power "focuses on short-range factors and neglects the deeper causes going back [to the economic settlement in] the Treaty of Versailles."[13] Kissinger's world view, while rooted in history, was so narrow and skewed when he came to power that it crippled him when he had to deal with the problems of the southern hemispheres.

For his part, Acheson fared little better in understanding those Third World affairs. From 1945 until late 1950 he was often confused about Asian problems. McClellan shrewdly argues that MacArthur did not march to the Yalu and go to war with China because, as Richard Neustadt and others would have it, the bureaucracy malfunctioned. It was a problem of perception: "The explanation seems to lie in the pervasive American tendency . . . to believe that somehow the canons of international politics are suspended when it comes to China." Acheson viewed the Peking regime as "a docile puppet of Moscow without a will of its own."[14] He paid little attention to Latin America and Africa, while his Indochina policy proved disastrous.

Acheson's policies in these areas did much to shape events in the 1950s and 1960s, and again Kissinger was a product of the times. By carefully planting the colorful "lone cowboy" image, Kissinger has successfully flummoxed a wide range of journalists and critics. By helping him build this image these observers have prevented us from coming to terms with conceptual failures that go far beyond a single actor. Kissinger, of course, seldom had to act alone. He enjoyed Fritz Kraemer's fatherly protection in the army, Carl Friedrich's and William Y. Elliott's powerful support at Harvard, Richard Nixon's alloyed blessings in Washington, and Nelson Rockefeller's influence and bankroll everywhere else. With support like that, Kissinger more resembled a cowboy riding along with the Third Army than a loner riding apart from the wagon train.

In 1968 Kissinger left Harvard to perform on the world stage, but in Mazlish's words, "It is clear that on most issues, at least till Watergate, Nixon called the tune."[15] Stoessinger's evidence, although not his thesis, substantiates that assessment, as does the testimony of some Kissinger aides who were with him during

[13] Mazlish, *Kissinger*, p. 292.
[14] McLellan, *Acheson*, p. 294.
[15] Mazlish, *Kissinger*, p. 217.

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the yeasty 1969 to 1971 years. Nixon originated many of the initiatives, but of at least equal importance, he provided the all-important political consensus required for the success of those initiatives. That he maintained that consensus until late 1973, long after it should have been destroyed by Vietnam, and that he did it in large part through lying and criminal acts, are also remarkable.

Equally noteworthy was Kissinger's insensitivity to the delicacy of that consensus, or, indeed, even to the need for devoting attention to the maintenance of a public consensus in American foreign policy. That insensitivity is exemplified in two well-known essays written before his move to Washington.[16] When the consensus finally disintegrated in 1973–1974, Kissinger was caught intellectually naked. The story goes that during a 1974 news conference he fended off criticisms by claiming that the great American "heartland" was with him. A reporter asked him how many times he had been to the "heartland" lately. Out of an inability to answer that question satisfactorily came Kissinger's plan to deliver a series of major addresses in the Midwest and South.

By that time, however, it was too late. The consensus which Truman and Acheson had carefully created with the Truman Doctrine had collapsed. A fundamental change began in the relationships between foreign and domestic policy, a change linking the two through pocketbook issues that created new constituencies at home with which the State Department had to deal. That change required, and continues to require, someone as secretary of state who is more than merely a technician, an experienced international negotiator, or a skilled administrator.

Throughout the first century of American government, the ablest secretaries of state (Jefferson, Madison, John Quincy Adams, Seward) were, not coincidentally, also powerful domestic political figures.[17] Since the central foreign policy issues involved commercial interests or expansion into foreign-held territory that abutted the American frontier, these men necessarily equated successful foreign policy with successful politics at home.

The relationship between foreign and domestic policy changed as the United States embarked upon its overseas expansion in the 1890s. Foreign policy became more particularistic and separated from most domestic concerns. The State Department, John Hay wrote during his tenure as secretary (1898–1905), was "remote from the public gaze and indifferent to it."[18] Hay characteristically exaggerated, but in 1909 *The Nation* remarked that an experienced congressman had told a new member "to avoid service upon a fancy committee like that of Foreign Affairs if he wished to make himself popular with his constituents, as they cared nothing about international questions."[19] That also turned out to be an exaggera-

[16] "Domestic Structure and Foreign Policy," and "Central Issues of American Foreign Policy," in Henry Kissinger, *American Foreign Policy. Expanded edition* (New York, 1974).

[17] A good discussion of this point can be found in Alexander DeConde, *The American Secretary of State, An Interpretation* (New York, 1962).

[18] Quoted in Peter F. Krogh, "The State Department at Home," *The Annals*, CCCLXXX (November 1968), 121.

[19] *The Nation*, September 30, 1909, pp. 294–295.

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tion, for in 1919 Woodrow Wilson failed the test of reconciling the public to his plans for a League of Nations.

During the 1920s, however, policy makers such as Charles Evans Hughes, aided by new groups as the Council on Foreign Relations, shrewdly created a fresh national consensus to support foreign policy. Another type of public involvement occurred in 1934 when the Reciprocal Trade Agreements Act suddenly created a tie between the State Department and domestic producers whose overseas interests were multiplying. By the eve of World War II, the department's number of economic personnel would equal its number of political officers. Such experienced observers as John Dickey, William Y. Elliott, and Don Price have agreed, in Price's words, that after the 1934 acts "no one could ever suppose again that our domestic and foreign affairs were separable."[20]

But Henry Kissinger, who learned much from Elliott on other matters, did make such a supposition. Or more accurately, he apparently believed that while they might be inseparable at points, foreign policies would not be ultimately governed by domestic affairs. His illusion probably resulted from Acheson's handiwork. More than any other official, Acheson performed a vital service for a generation of foreign policy makers by devising in the Truman Doctrine a Manichean world view that, when firmly fixed in the American mind, gave those policy makers a freedom of action that nineteenth-century secretaries of state would have found unbelievable. Acheson created this consensus, and the next four administrations worked hard to maintain it, partly out of a fear that a free-wheeling public opinion would undermine foreign policy, and also out of a certain disdain for the right of Congress and the larger public to shape policy. Acheson resembled Kissinger, not Seward, Madison, or Hughes, in calling a presidential nominating convention "a mad and not a little degrading spectacle," in terming his job as State Department liaison with Congress "a low life but a merry one," and in causing one congressman to complain that when testifying before a Capitol Hill committee Acheson often looked "as if a bit of fish had got stuck in his mustache."[21]

McClellan shares many of Acheson's views, consequently overlooking evidence, including public opinion polls which circulated in the State Department during 1947, that revealed a remarkably sophisticated public which at least intuited the dangers of global military involvement far better than did Acheson or his successors. But McClellan candidly admits that Acheson and Truman "certainly contributed a stick or two" to the "fires" that reduced American perception of world affairs to a burned-out Manicheanism. The secretary of state, McClellan concludes, blurred "strategic distinctions between vital and peripheral interests. . . . Thus his efforts to explain limited politics in limited terms did not succeed."[22] In other words, he became the captive of his own supposed success.

[20] Price, *The Secretary of State*, pp. 144, 168.
[21] McClellan, *Acheson*, pp. 20–23; *Harper's*, CCXI (November 1955), 20.
[22] McClellan, *Acheson*, pp. 133–134, 404.

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Kissinger apparently understood little of this, or perhaps saw no need to question it since he agreed with both of Acheson's priorities: containment of communism everywhere at all costs, and the subordination of domestic needs and politics to the containment policy. Mazlish perceptively notes that Kissinger "postulates and assumes a conservative domestic policy as a sort of given, a necessary correlate to a conservative foreign policy."[23] When the status quo crumbled under the attacks of civil rights forces and the antiwar movement, Kissinger urged a new consensus which would once again bring foreign policy debate to a halt considerably short of the water's edge. Others argued that Congress' role had to change (and, by implication, the secretary of state's role as a political leader had to be rethought) because of the decay in the two-party system. In Joseph Califano's words, "Partisan politics have thus had little relevance to the conduct of foreign policy for more than thirty years."[24]

That apparent historical fact, however, will not excuse secretaries of state from acting, as nineteenth-century secretaries acted, as political partisans. For partisanship means more than competition between the two major parties. In many of the most momentous foreign policy debates during the past seventy years, partisan divisions were intraparty: the Roosevelt-Taft division over arbitration treaties and Asian policy, the Hoover-Borah split over approaches to Japan and the Soviet Union, the Hull-Morgenthau argument over policy toward Japan and Germany, the Truman-Wallace fight over containment, the Eisenhower-Taft struggle over foreign commitments, the Johnson-Fulbright (or McCarthy or McGovern) falling-out over Vietnam, and the Kissinger-Reagan clash over détente and Panama. The "party may be over," but the politics linger on. That is why William Y. Elliott was right when he noted in 1960 that "The idea of having a Cabinet Secretary (of any department) who is 'nonpartisan' contradicts the logic of Presidential government."[25]

That is also why one of the best recent essays on the secretary of state's role focused on its political responsibilities. Since 1945, Howard Furnas observed, "foreign policy programs have been largely argued for and justified solely in foreign policy terms." In the future such arguments would be "clearly incomplete," if not "irrelevant," because they must increasingly take domestic priorities into account. A Foreign Service officer since 1947, Furnas understood that "The nation does not, after all, exist in order to conduct foreign affairs. It is not, after all, a bad thing for the American government to reflect the sense of priorities felt by the American people." At pivotal points in their careers, Acheson and Kissinger would have disagreed with that statement. It is a measure of the change that has occurred, however, that few would disagree with Furnas' assertion that henceforth the secretary of state would have to compete with domestic needs for national resources. Furnas consequently urged that future secretaries

[23] Mazlish, *Kissinger*, pp. 154–155.
[24] Joseph A. Califano, Jr., *A Presidential Nation* (New York, 1975), p. 154.
[25] Price, *The Secretary of State*, p. 116.

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should be of presidential caliber since such a figure could more easily gain support from the public, and "most important of all, he would in his bones understand and know how to meet the President's need for a foreign policy more closely related to the broader national scene."[26]

Bayless Manning, president of the Council on Foreign Relations, has also emphasized that decision making must occur at the intersection of domestic and foreign policies, or when, in his word, these policies become "intermestic." Instead of following Furnas' suggestion, however, Manning proposed institutional changes, including an International and Domestic Affairs Council, comprised of various cabinet members, which would resemble the National Security Council and have a new presidential special assistant acting as chairman when the president and vice-president were absent. Policy integration would occur at this point.[27]

Any such change should be considered carefully. Manning's proposal is one of the council's several initiatives that have sought to streamline bureaucratic decision making at the possible expense of political values. The executive does not need another presidential special assistant who would, among other objectionable actions, take decision making further into the bowels of the White House and out of the hands of a secretary of state who at least must give some accounting before congressional committees. The State Department, moreover, is the agency which can most effectively mobilize the information and cosmopolitan views necessary to discipline and subordinate particularistic interests, a process which Manning deems essential. Solving "intermestic" problems will be better accomplished, as Furnas suggested, by a secretary of state who can conceptualize those problems and is politically sensitive to public and congressional concern, not by another hidden White House authority.

The heart of the matter will be the secretary of state's ability to obtain congressional and broad public support for his policies, particularly those relating to explosive Third World areas. Appointing a secretary of state who has no political constituency and acts mostly as a diplomatic troubleshooter may make politics simpler for the White House, but it was not the way Washington, Jefferson, Monroe, Lincoln, Franklin D. Roosevelt, or even Warren G. Harding ran the government. Nor can it be the way now, for the wishbone has nearly pulled apart: Acheson's consensus has deteriorated, Kissinger has failed to rebuild it, and little-known but increasingly powerful parts of the world are demanding a different distribution of American wealth.

[26] Howard Furnas, "The President: A Changing Role?" *The Annals*, CCCLXXX (November 1968), 9–15.

[27] Bayless Manning, "The Congress, the Executive and Intermestic Affairs: Three Proposals," *Foreign Affairs*, LV (January 1977), 306–324.

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How Conservative Are American Catholics?

ANDREW M. GREELEY

It has become part of the conventional wisdom that, while they traditionally used to vote Democratic, Catholic ethnics are now conservative. They are more likely to be racist, less likely to support civil liberties, more likely to take a punitive attitude toward the counterculture. They were stronger supporters of the Vietnam war than other Americans, and voted heavily for Wallace in 1968. As many of them moved into the suburbs and became more affluent, they began to drift away from the Democratic party in both affiliation and voting behavior.

Although this description of the voting behavior and views of American Catholics seems unexceptionable, it happens to be demonstrably untrue. The remainder of this article will present empirical data showing that this "accepted" view is false and will offer a more accurate and realistic account of how Catholics have been participating in the American political system.[1]

[1] Other parts of the conventional wisdom about Catholics include the following propositions: (1) Catholics tend to be lower middle class blue-collar workers. Educationally and financially they do not compare with their Protestant counterparts. Irish Catholics, who have been in the United States longer than other ethnic groups, have achieved a certain amount of modest respectability; but they have not made the most of their opportunities. (2) Most Catholic priests are not happy in their vocations. The commitment to celibacy makes it impossible for a man to

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Realignments of American Politics

A principal theme of American political analysis for the last decade has been realignment—the emergence of new political coalitions and new distributions of political power. Apparently this realignment process is continuing, but no definitive realignment has yet taken place to create a new majority party. As my collegues Sidney Verba, John Petrocik, and Norman Nie pointed out in their book, *The Changing American Voter*,[2] some realigning has taken place. The South has become much more Republican, the Northeast much more Democratic than either section was in the past. Blacks, already heavily Democratic, have become more so; blue-collar Protestants, once moderately Democratic, have become somewhat less Democratic. Independents are more numerous now than they have been for a long time previously, not so much because of a switch away from either of the traditional parties as because a whole new generation of politically unmobilized voters have become adults in the last fifteen years. This younger generation is much less inclined to vote than its elders and is as yet politically unaffiliated—very like the political unaffiliated of the 1920s. The "realignment" of 1932 occurred not so much because people stopped being Republicans and became Democrats (though in later years this apparently did happen with some black adults) but rather because the previously unmobilized younger generation of voters who grew up in the 1920s became Democratic in the Roosevelt election. Whether such a mobilizing election will occur in the next decade and perform a similar function for the political unaffiliates of the present era remains to be seen. There is a potential for realignment in this younger generation, and it does not seem to be leaning toward the Republicans. Thus it appears that at this time the realigning moves that have occurred tend to cancel one another out; the Democrats continue to be the majority party, as they have been since 1932. Barring the emergence of a third party that would mobilize much of the young vote or a dramatic shift of this unaffiliated vote to the Republican side, the Democrats will continue to be the larger of the two political parties.

But before Nie, Petrocik, and Verba produced their very careful analysis, a number of observers such as Kevin Phillips,[3] William Gavin,[4] Patrick Buchan-

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develop capacities of openness, intimacy, and sympathy with human frailty. Most Catholic clergy would marry given the chance. (3) While Catholics are more likely to be against abortion than Protestants, the encyclical letter *Humanae Vitae* on birth control caused serious moral anguish for large numbers of American Catholics. (4) Catholic support for their parochial schools is declining, mostly because Catholics realize that in the suburbs where they now live the public schools are better. Also, there is no evidence that parochial school graduates are any more religious than public school graduates. Parochial school gradutes, however, are more likely to have hostile attitudes toward blacks and Jews, and less likely to be well equipped for success in American society. For the evidence that each of these propositions is also false, see Andrew M. Greeley, *The American Catholic: A Social Portrait* (New York, 1977).

[2] Norman H. Nie, John R. Petrocik, Sidney Verba, *The Changing American Voter* (Cambridge, Mass., 1976).

[3] Kevin Phillips, *The Emerging Republican Majority* (Garden City, N. Y., 1969).

[4] William F. Gavin, *Street Corner Conservative* (New York, 1975).

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an,[5] and William Rusher[6] described a "new Republican majority" which would include the economic conservatives (the old right wing of the Republican party) plus the social conservatives who were thought to be for the most part "the Catholic ethnics" who object to the "far out" liberalism of the left wing of the Democratic party. Those Catholic ethnics, in other words, who were lost to the Democrats during the McGovern candidacy in 1972 were viewed as potential components of a new Republican coalition. This feeling has been echoed just as consistently from the Democratic left, which has always been embarrassed by its Catholic ethnic allies, and particularly by the big-city urban "bosses" like the late Mayor Richard Daley and union "bosses" like George Meany. The intellectuals who designed the grand strategy of the McGovern movement—the Galbraiths, the Burns, the Duttons—thought that the Catholic ethnics could be conceded to the Republican party "where they belonged," and their departure could be made up for by the "legions of the young," who had come to voting age during the troubled Vietnam years. Unfortunately for their grand design, the majority of those under thirty were not like the students these worthy professors saw in their classrooms; like their parents they were more inclined to vote for Nixon than for McGovern. Even among those under twenty-five, McGovern squeezed out a bare plurality. He did quite well with the college young, but the majority of American young people (55 percent) do not go to college, and in that group Nixon won overwhelmingly.

Since the late 1960s, then, there has been an assumption among right-wing Republicans that the Catholic ethnics "belonged" with them and an assumption among Democrats that they did not need the Catholic ethnics. Both assumptions seem to have been wrong thus far. The McGovern movement went down to Goldwater-like defeat—in part because it lost substantially among Catholic voters; but the Republican party has not been able to capture any substantial support among Catholics and continues to be a party of about one-quarter of the country—a very homogenous one-quarter at that, predominantly white middle-class Protestant.

Social Conservativism and Economic Conservativism

It does not seem to have occurred to writers like Rusher, Phillips, or Gavin that they may have misunderstood the conservative bent of urban Catholics, and that there may be considerably more distance between the so-called social conservatives and the economic conservatives than these architects of a new conservative coalition are inclined to believe. Similarly, it has yet to occur to many liberal left Democratic idealogues that Catholic ethnics, along with union members, Catholic and Protestant, are still an absolutely indispensable partner in any liberal coalition. Nevertheless, the evidence assembled in this article indicates that

[5] Patrick Buchanan, *The Conservative Choice* (New York, 1975).
[6] William A. Rusher, *The Making of a New Majority Party* (New York, 1975).

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the so-called social conservatives are much further away both socially and economically from the economic conservatives than either the conservatives or the liberals are prepared to believe.

Catholics have voted Democratic since the nineteenth century, when the Democratic party was able to make alliance with the new immigrants in the face of the nativist hostility of much of the Republican party. Despite ups and downs in this alliance (and the most severe down came at the end of World War I when Woodrow Wilson appeared to many Catholics to be violently anti-Catholic), the urban ethnics and the Democratic party have been firmly aligned. Such issues as Prohibition strengthened the alliance, and, as the latest immigration of eastern and southern Europeans came of voting age in the 1920s, they inclined toward the Democratic party first on the Prohibition issue and then because of the Al Smith candidacy. The Roosevelt New Deal in 1932 strengthened the alliance between Catholics and the Democratic party, and from 1932 to 1972, the Democratic candidates could always count on a minimum of three-fifths of the Catholic vote in the presidential elections. (The proportion fell only slightly below that even in the Republican landslide election of 1956.)

However, it has been argued by both the left, which wants the Catholics out of the Democratic party, and the right, which wants Catholics in a new Republican conservative coalition, that Catholics have been drifting away from the Democratic party because of their immigration to the suburbs and because of the increased importance of "social" issues (which would include such issues as race, marijuana, amnesty for Vietnam draft dodgers, abortion, etc.). Catholics were still voting Democratic, it might be admitted, but they were less likely to do so now. They were less likely to define themselves as Democrats, and their political attitudes were drifting to the right as the Democratic party drifted to the left The data assembled here, however, provide little support for such a view. Catholics have not severed their affiliation with the Democratic party; they are still substantially more likely than other Americans to vote Democratic in both presidential and congressional elections, and their 1972 defection was not disproportionate to that of other Democrats; and their political and social attitudes put them on the liberal side of the American political spectrum.

Of course, a good deal depends on how you define "liberal" and "conservative." Most Americans are not too consistent in their political and social attitudes; one achieves only a fair degree of predictive success if one tries to estimate, let us say, a given person's attitude on foreign policy from his attitude on welfare reform. It is true, as Nie, Petrocik, and Verba have demonstrated, that political consistency has increased considerably in the last two decades (apparently as part of one of the great flings upward and downward in consistency that occurs periodically in American history). But still, the overwhelming majority of Americans are neither consistently liberal nor consistently conservative on all salient political issues. If to be a "liberal" one has to hold the official liberal position on every issue, then not very many people in this society are liberal; the majority of American Catholics—unlike the majority of the Jews and blacks

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—would not fall on the liberal *end* of the political spectrum. Furthermore, if one has to endorse the counterculture style and its "new morality," then not only most Catholics but also most Jews and blacks are not liberal. However, if liberalism means support for the New Deal "bread-and-butter" social stance, then the overwhelming majority of Catholics are still politically liberal. On almost all the issues that are pertinent in American political life today, Catholics are more likely than Protestants to fall on the liberal side of the political spectrum.

Party Affiliation and Voting

Since the work of Scott Greer[7] on the St. Louis metropolitan area, it is generally assumed that as the Catholic ethnics make money and move to the suburbs, they tend to weaken their ties with the Democratic party. Such ties are considered to be the product of both poverty and of the tight constraints of intercity ethnic neighborhoods. It occurred to very few observers, however, that St. Louis, with its strong German population, might not be the typical American urban setting. Using data collected in the University of Michigan's national election studies over the past two decades, my colleague Joan Fee[8] attempted a very complex and sophisticated analysis of the question of the possible disaffiliation from the Democratic party of Catholics as they became better educated and more suburbanized. Fee deals with voters at three points in time, an early period (1952, 1956, 1958), a middle period (1960, 1962, and 1964), and a late period (1968, 1970, 1972). Each period contained two presidential elections and one congressional election. In time 1, 42 percent of the Catholic population lived in the suburbs, by time 3, 68 percent of the Catholics lived in the suburbs; but there was virtually no change in the proportion of Catholics identifying with the Democratic party. There was a decline of party identification because younger Catholics, like all other younger people, are more likely to be independent; but among party affiliators, there was little Catholic erosion from the Democratic party in the two decades. There was virtually no difference in Democraticy party affiliation between suburban Democrats and those still living in the central city.

Analyzing the same national election file used by Fee, John Petrocik[9] found that on a scale made up of six political opinion items (welfare, race, cold war, school integration, and size of government), Catholics were on the average slightly left of center in the late 1950s and still left of center at the end of the 1960s—although there had been a slight movement to the right during the two decades. However, the proportion of Catholics at the far liberal end of the political continuum had actually increased during the period Petrocik studied.

[7] Scott Greer, "Catholic Voters and the Democratic Party," *Public Opinion Quarterly*, 25 (1961), 611–625.

[8] Joan Fee, "Political Continuity and Change," in Andrew M. Greeley, William C. McCready, and Kathleen McCourt, *Catholic Schools in a Declining Church* (New York, 1976), pp. 76–102.

[9] John R. Petrocik, "Changing Party Coalitions and the Attitudinal Basis of Alignment, 1952-1972," Ph.D. diss., University of Chicago, 1975.

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When we turn from affiliation to voting, one can see in Table 1 that the four major Catholic ethnic groups voted in overwhelming numbers for Democratic congressional candidates between 1952 and 1970. With the exception of the German Catholics, there was virtually no difference in the 1970 Democratic congressional vote than there was in the two-decade average. Furthermore, there was virtually no difference for each of the ethnic groups in the proportion voting for Hubert Humphrey when compared with the average proportion voting Democratic in the presidential elections of the 1950s and 1960s. The Poles, who were supposed to have voted for Wallace in large numbers, were even more likely to vote for Humphrey (80 percent) than their two-decade Democratic presidential average (which was still a very high 76 percent). Each of the Catholic ethnic groups was more likely to vote for George McGovern in 1972 than was the national American average. And the Gallup Poll showed that during the Watergate crisis Catholics turned against Nixon earlier and more strongly than did other Americans.

Ideological Positions

It was during the late 1960s and the early 1970s that the image of the white ethnic hard-hat racist polluter hawk seemed to grip the editorial writers, columnists, and feature story writers in the national journals. However, in the national survey data of the time, there was almost no confirmation for this image. In Table 2 one can compare the Italian and Polish ethnics with the Anglo-Saxon Protestants. One notes that the order of importance of the first five issues listed is precisely the same: war, marijuana, inflation, crime, and pollution. However, the Italians and the Poles were more likely to see each of these as very serious problems than were Anglo-Saxon Protestants. More than half of both groups thought that pollution, inflation, and the war were very serious problems. However, as of spring 1971, three-fifths of the Italians and of the Poles thought that

TABLE 1

Where Ethnics Stand on Voting Patterns and Party Affiliation

	Irish Catholics	*German Catholics*	*Italian Catholics*	*Polish Catholics*	*All Americans*
Average percent voted Democratic Congress, 1952–1970	70	57	62	76	55
Average percent voted Democratic, 1970	73	45	62	73	54
Average percent voted Democratic for president, 1952–1968	65	55	60	76	48
Average percent voted for Humphrey, 1968	65	55	59	80	44
Percent Democrat or Democrat-leaning, spring 1972	61	57	63	72	55

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TABLE 2

Where Ethnics Stand on Rank Order of First Six "Very Serious Problems," NORC 1971

Rank of Problem	Anglo-Saxon Protestants		Irish Catholics		German Catholics	
	Problem	%	Problem	%	Problem	%
1.	War	68	War	69	War	62
2.	Marijuana	51	Inflation	55	Inflation	49
3.	Inflation	48	Crime	52	Pollution	46
4.	Crime	44	Welfare	42	Marijuana	42
5.	Pollution	42	Unemployment	38	Crime	39
6.	Unemployment	30	Pollution	38	Unemployment	30

Rank of Problem	Italian Catholics		Polish Catholics	
	Problem	%	Problem	%
1.	War	73	War	71
2.	Marijuana	60	Marijuana	60
3.	Inflation	57	Inflation	57
4.	Crime	55	Crime	55
5.	Pollution	54	Pollution	55
6.	Welfare	53	Urban unrest	35

marijuana was a very serious problem in the country, putting it in second place on their list of problems, as did Anglo-Saxon Protestants.

Table 3 gives the proportions of respondents in each group who both liked and disliked various elements in the society. The overwhelming majority of all the groups were sympathetically disposed to college students, indicating that the younger generation is by no means despised by most Americans. The Irish, Italians, and Poles are somewhat less likely, however, to like college students than are Anglo-Saxon Protestants. Similarly, while the majority of all the groups like college professors, the Irish and the Italians are somewhat less inclined to like them, and only a minority of the Poles (40 percent) are willing to admit that they like college professors. But while students and professors get good marks from the ethnics, radical students get very poor marks with more than four-fifths of each group (and more than four-fifths of the Anglo-Saxon Protestants concur) registering dislike of radical students.

Admiration for big business is less among the Catholic ethnics than it is among Anglo-Saxon Protestants, although only the Italians are notably more inclined to like labor unions. Liberals enjoy a better image with the Irish than they do with the other five groups, though only the Italians are less sympathetic to the liberals than are Anglo-Saxon Protestants. Similarly, the German, Italian, and Polish Catholics are somewhat less likely to have a favorable image of conservatives than are Anglo-Saxon Protestants.

There is little difference between the Anglo-Saxons and the Germans, Ital-

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TABLE 3

Where Ethnics Stand on Their Likes/Dislikes

Object of Likes/Dislikes	*Anglo-Saxon Protestants*	*Irish Catholics*	*German Catholics*	*Italian Catholics*	*Polish Catholics*
College students	80/3%	67/0%	80/2%	70/0%	72/6%
Big business	76/14	42/21	31/26	57/10	33/21
Liberals	27/27	39/28	31/26	15/25	28/21
Blacks	50/12	79/0	49/7	50/2	46/13
Conservatives	46/12	46/18	30/13	38/8	31/13
Hippies	7/77	14/58	3/70	3/66	2/79
Radical students	4/86	7/86	2/81	14/82	0/88
Welfare recipients	30/21	28/17	33/28	28/18	10/35
College professors	63/7	57/10	67/3	57/7	40/2
Police	87/3	86/0	91/5	90/0	78/4
Unions	43/33	43/21	46/30	50/19	45/20
Black militants[a]	2/83	0/96	2/83	5/78	4/86

[a]The black response to this question is 35/37%.

ians, and Poles in their image of blacks. The Irish, however, are notably more inclined to have a favorable image of them. All groups have a very unfavorable image of black militants. The Irish, German, and Italians have about the same image of welfare recipients as do the Anglo-Saxon Protestants, although the Polish image is distinctly unfavorable. Finally, all five groups are very favorably disposed to the police.

The ethnics are not "pure" liberals, and indeed a substantial proportion of them seem to dislike "pure" liberalism. Table 4 suggests that there is a strong reserve of potential support among the ethnics for liberalism, however. The majority of the Irish, for example, favored withdrawal from Vietnam, stiff punishment for pollution, speeding up racial progress, government support for family assistance, and a maximum use of resources to eliminate poverty. On each of these issues, the Irish are more "liberal" than Anglo-Saxon Protestants. A surprisingly large minority of the Irish (30 percent) were also willing to support the legalization of marijuana (perhaps because the Irish romance with John Barleycorn makes them reluctant to deny any kind of narcotic to others). The Irish are also considerably more likely than the Anglo-Saxon Protestants to support government pressure for neighborhood integration and to oppose punishment for student rioters, although in both cases only a minority of the Irish respondents took such a position. On the other hand, 51 percent of the Irish respondents favored the use of force to end urban unrest. Finally, the Irish are the most likely of all the groups under consideration to think that the courts have gone too far in protecting the rights of criminals.

A majority of the German Catholics take the "liberal" side only on stiff penalties for pollution, though Germans are more likely to be on the liberal side

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TABLE 4

Where Ethnics Stand on Solutions to Issues, NORC 1971

Issue	*Anglo-Saxon Protestants*	*Irish Catholics*	*German Catholics*	*Italian Catholics*	*Polish Catholics*
Vietnam: withdrawal/victory	40/36%	64/32%	46/28%	53/32%	41/30%
Pollution: fines and jail/ nothing	71/19	77/23	85/4	83/4	79/11
Riots: solve problems/force	47/38	35/51	58/28	40/52	59/29
Racial progress: speed up/ slow down	40/32	57/19	32/27	43/40	22/35
Family assistance: government support/support self	37/40	42/24	42/25	55/30	30/65
Marijuana: allowed/not allowed	8/87	30/52	21/69	12/88	13/79
Rights of criminals: more careful/too far	27/58	13/73	19/64	33/59	17/68
Forced neighborhood integration: government should/should not	22/67	34/49	27/55	28/55	22/23
Student rioters: should not punish/punish severely	11/74	20/67	8/74	23/59	10/75
Government to eliminate poverty: all resources/has done too much	43/37	66/17	45/31	62/31	74/11

than Anglo-Saxon Protestants on the war, on solving the underlying problems that cause riots, on family assistance, marijuana, and neighborhood integration.

Italian Catholics provide majority support for the liberal response on the war, pollution, family assistance, and government efforts to eliminate poverty; and they are also more concerned than any of the other groups for the protection of the rights of criminals and the nonpunishment of student rioters.

Finally, even though the Poles were the least likely of the ethnic groups to espouse a "liberal" position, 79 percent of them favored strong punishment for pollution, and 74 percent (the highest of any of the five groups) favored all possible government resources used to eliminate poverty.

Using data collected at about the same time, Professor James Wright observed in an article in *The Nation*[10] that Catholics were less likely to own guns than Protestants. Among non-South whites, living in cities or suburbs, 50 percent of the Protestants own a gun and 25 percent own a pistol, while only 25 percent of the Catholics own a gun and 10 percent own a pistol. More than 90 percent of

[10] James Wright, "Who Owns the Side Arms," *The Nation*, September 20, 1975, pp. 240–244.

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TABLE 5

Attitudes on Certain Political Issues

Attitude	*Northern White Blue-Collar Workers*		*National Average*
	Protestant	*Catholic*	
In favor of 18-year-old vote			
1968	62% (217)	65% (150)	66%
1969	62 (221)	64 (129)	61
In favor of basing welfare on cost of living	79 (191)	79 (136)	77
In favor of guaranteed annual wage	29 (228)	47 (134)	32
Would vote for a black president	72 (193)	78 (142)	67
Integration moving too fast	57 (280)	42 (127)	48
Deeply concerned about pollution	50 (258)	55 (110)	51

SOURCE: Gallup Polls.

the Catholics who do not own guns support gun control, as do more than three-fifths of the gun owners. Two-thirds of the Anglo-Saxon Protestant population supports gun control, as does 85 percent of the Irish Catholic, 72 percent of the German Catholic, 81 percent of the Italian, and 74 percent of the Polish Catholic groups. Thus the image of the armed and dangerous white ethnic, ready to shoot on sight any hippie or black who threatens his neighborhood may have a mythological utility to certain other segments of American society, but it scarcely corresponds to the facts.

During the same late 1960–early 1970 period, the so-called blue-collar ethnic supported the eighteen-year-old vote by a two-thirds majority; this group also supported basing welfare on the cost of living by more than a three-quarters majority, and more than half were deeply concerned about pollution. Three-fourths would vote for a black president, and more than half rejected the proposition that integration was moving too fast. Almost half of this same population would favor a guaranteed annual wage—almost 15 percentage points higher than the national average (Table 5).

It is interesting to note that blue-collar workers, including Protestants, were more likely than the national average to favor withdrawal from Vietnam within eighteen months in February of 1970. It is also worth noting that white-collar Catholics were substantially more likely to be in favor of withdrawal than were white-collar Protestants, and that Catholic professionals and executives were the most likely of any group shown in Table 6 to be opposed to the war.

The data in Table 6 were collected in 1970, when it was permissible and even fashionable to be against the war. However, on the dove scale constructed by Sidney Verba and his colleagues at Stanford in their 1967 study of attitudes on the Vietnam war (Table 7),[11] the ethnics were substantially more likely to be at

[11] Sidney Verba et al., "Public Opinion and the War in Vietnam," *American Political Science Review*, 61 (June 1967), 317–333.

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TABLE 6

Attitudes toward the War, February 1970
(Percent in favor of withdrawal immediately or within 18 months)

	Northern Whites	
Occupational Group	*Protestants*	*Catholics*
Blue collar	50% (195)	47% (114)
Clerical, sales, service	32 (73)	38 (60)
Professional and executive	39 (160)	52 (101)
National average	46%	

the dove end of the scale than were native American Protestants. Only the eastern European Catholics were lower on the dove scale than were native American Protestants. In other words, three of the four ethnic groups were not only *not* more hawkish in 1967, they were less hawkish than native Americans. One may wonder why so few of the leaders of the peace movements were aware of white ethnic antipathy toward the war, and why so many were eager to write off white ethnics as hard-hat hawks. The ethnics cannot be expected to hate a society which has made it possible for them to have the kind of life their parents, grandparents, and great-grandparents would never have dreamed possible. A position toward the war that appealed to patriotism instead of ridiculing it might have been effective among them.

In a 1973 political attitude study carried out by Professors Nie, Verba, and Petrocik, evidence was found to confirm and complete this picture of Catholics as moderately liberal on "New Deal" social issues. Catholics, whether they are Democrats, independents, or Republicans, are generally more likely to support the traditional New Deal social reforms—government intervention, government support of employment, income equalization, and Medicare—than are their Protestant counterparts. Indeed, there is substantially less correlation on the first two items between affiliation and attitude for Catholics than there is for Protestants. The proportion of Catholic support declines only slightly as one

TABLE 7

Attitudes of American White Ethnic Groups toward the Vietnam War, 1967

Group	*Dove*
Jewish	48% (47)
Western European Catholic	29 (149)
Southern European Catholic	26 (60)
Western European Protestant	17 (499)
Native American	15 (104)
Eastern European Catholic	7 (42)

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TABLE 8

Attitudes on Party Affiliation and Population Group for 1973 on Traditional "New Deal," Civil Liberties, and Racial Issues—Percent "Liberal"

A. Traditional "New Deal" Issues

Socioethnic Group	Jobs			Government Intervention			Income Equalization			Medicare		
	Democratic	Independent	Republican	Democratic	Independent	Republican	Democratic	Independent	Republican	Democratic	Independent	Republican
White northern Protestant	50	38	31	38	35	22	64	63	40	72	84	73
White border state Protestant	52	28	22	32	14	24	64	28	48	75	71	57
White southern Protestant	47	30	25	38	20	23	52	49	41	75	79	61
Catholic	58	50	49	46	37	39	63	67	54	84	79	66
Jews	55	—[a]	—	60	—	—	72	—	—	95	—	—
Blacks	86	—	—	68	—	—	86	—	—	92	—	—

B. Civil Liberties Issues

Socioethnic Group	Marijuana			Pornography			Strict on Criminals			Spy on Radicals		
	Democratic	Independent	Republican	Democratic	Independent	Republican	Democratic	Independent	Republican	Democratic	Independent	Republican
White northern Protestant	35	32	17	40	41	31	27	26	19	54	51	38
White border state Protestant	7	14	18	30	14	23	43	14	27	47	43	28
White southern Protestant	10	13	20	26	47	19	26	20	27	43	39	39
Catholic	28	30	20	31	43	35	33	23	24	48	46	49
Jews	72	—	—	70	—	—	55	—	—	92	—	—
Blacks	44	—	—	41	—	—	52	—	—	59	—	—

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TABLE 8 (continued)

C. Racial Issues

Socioethnic Group	*Black Welfare*			*School Integration*			*Housing Integration*		
	Democratic	*Independent*	*Republican*	*Democratic*	*Independent*	*Republican*	*Democratic*	*Independent*	*Republican*
White northern Protestant	27	18	17	34	17	27	35	34	18
White border state Protestant	35	29	16	42	29	37	34	43	24
White southern Protestant	14	17	18	18	13	23	17	13	19
Catholic	30	35	12	36	49	24	34	24	23
Jews	40	—	—	52	—	—	50	—	—
Blacks	80	—	—	76	—	—	82	—	—

[a]Insufficient Jewish and black independents and Republicans to give percentage.

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moves from right to left across Table 8. There is, however, generally relatively little difference between northern Catholics and northern Protestant independents and Republicans on such civil liberties issues as marijuana, pornography, strictness against criminals, and spying on radicals.

On racial issues such as black welfare and compulsory school and housing integration, the majority of Americans do not side with the liberal coalition, but Catholic independents are more likely to support promotion of black welfare and compulsory school integration than their northern Protestant counterparts. They are also more likely to support such programs than are Catholic Democrats. Indeed, among white Gentiles, Catholic independents have the highest proportion of support for both the welfare and the integration questions (Table 8–C).

Elections will still be won or lost in the United States on the traditional New Deal issues. Marijuana and pornography are matters that will be worked out by the state legislatures and the courts. The American public favors equality of opportunity, but it does not like quotas, reverse discrimination, compensatory justice, affirmative action, or whatever else such policy might be called. But most Americans are not personally affected by either busing or quotas, and they will not make such issues decisive in elections unless and until they are personally affected by them. In any event, one can conclude from Table 8 that the liberal coalition which is currently dominant in the structure of the Democratic party (made up mostly of blacks, Jews, and very high-status northern white Protestants) is likely to find that on both the traditional New Deal issues and on the newer racial issues, the strongest potential support will come from that group of independents whose religious background would predispose them to be part of the Democratic coalition.

There is, then, no evidence to sustain the hoary image of the right-wing, anti-civil-libertarian Catholic. In fact, Catholics are at least as libertarian as Protestants, and in most cases, somewhat more so (Table 9). For example, Irish Catholics are the most likely of all to endorse the right of the socialist to speak, and they are first among all the Gentile groups to uphold the speaking and teaching rights of atheists and communists. They are also the most likely of the Gentile groups to defend the appearance of a communist book in the public library. Their Italian coreligionists are generally the second most liberal of the Gentile groups, though they are ahead of their Irish counterparts in their support for the right of books by atheists to appear in the public library—and here their support is even higher than that of Jewish respondents. Polish and Slavic Catholics generally are indistinguishable on civil libertarian issues from the mainstream "other" or "American" Protestant groups, despite the presumed hostility of these eastern European groups to the Communist regimes that dominate their mother countries.

Conclusions

To summarize the state of our knowledge, the Catholic marriage with the Democratic party has not come to an end. Catholics are not leaving the Democratic

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TABLE 9

Civil Liberties Attitudes—NORC General Social Surveys 1972, 1973, 1974
(Percent metropolitan North only)

	Atheist[a]			*Socialist*[b]			*Communist*[c]		
Ethnic Group	*Speak*	*Teach*	*Book*	*Speak*	*Teach*	*Book*	*Speak*	*Teach*	*Book*
Protestant									
British (218)	73	43	55	89	63	54	64	36	69
German (247)	65	40	56	78	56	60	60	40	64
Scandinavian (74)	69	42	55	80	60	54	57	37	62
Irish (102)	66	43	52	74	52	57	57	63	59
Other (316)	64	37	52	74	53	58	53	34	56
Catholic									
Irish (113)	81	57	52	89	70	53	76	50	71
German (86)	77	51	57	81	61	62	66	38	62
Polish (71)	65	41	44	77	52	46	49	33	55
Slavic (34)	67	41	50	73	64	49	53	41	55
Italian (146)	77	50	59	84	64	56	64	45	59
Jew	83	68	54	87	77	58	80	60	83

[a]*Atheist attitude question:* "There are always some people whose ideas are considered bad or dangerous by other people. For instance, somebody who is against all churches and religion . . .

A. If such a person wanted to make a speech in your city (town, community) against churches and religion, should he be allowed to speak, or not?
B. Should such a person be allowed to teach in a college or university, or not?
C. If some people in your community suggested that a book he wrote against churches and religion should be taken out of your public library, would you favor removing this book, or not?"

[b]*Socialist attitude question:* "Or consider a person who favored government ownership of all the railroads and all big industries.

A. If such a person wanted to make a speech in your community favoring government ownership of all the railroads and big industries, should he be allowed to speak, or not?
B. Should such a person be allowed to teach in a college or university, or not?
C. If some people in your community suggested a book he wrote favoring government ownership should be taken out of your public library, would you favor removing this book, or not?"

[c]*Communist attitude question:* "Now, I should like to ask you some questions about a man who admits he is a Communist.

A. Suppose this admitted Communist wanted to make a speech in your community. Should he be allowed to speak, or not?
B. Suppose he is teaching in a college. Should he be fired, or not?
C. Suppose he wrote a book which is in your public library. Somebody in your community suggests that the book should be removed from the library. Would you favor removing it, or not?"

party, despite their notable increase in education and their greater propensity to live in the suburbs. Up to the 1972 presidential election, they consistently voted for Democratic presidential and congressional candidates by a 60 percent or better majority. Their defection from the Democratic camp in 1972 was transitory, and their political beliefs and practices have not grown notably more conserva-

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tive in the last two decades. Catholics are about where they were twenty years ago in their political and social attitudes, somewhat to the left of center. They are logical partners in any liberal coalition. If they are compared to Jews, university professors at elite universities, and to writers of the national media, as well as to other such elite groups, they are conservative, but then so is 90 percent of the country. But if the liberal-left elite of the Democratic party is looking for a coalition partner with which to win elections (which is what politics is about—or at least used to be about), then there is simply no other group in American society to look to except Catholics. The power of mythology, of unexamined symbolic assumptions, of pictures deeply rooted in the unconscious that blind one to reality and turn potential allies into imaginary enemies must be very great indeed to keep alive the fiction of the blue-collar conservative ethnic.

We have argued elsewhere that the ethnics are, if anything, less blue-collar than the American average. Now we argue that they are less conservative than the American average. The evidence is so overwhelming as to be incontrovertible; and yet there are many peope who simply do not want to believe it, although if they could accept and utilize it, their return to and continuation in political power for the rest of the century would appear assured. Unconscious imagery and unexampled assumptions, as well as unacknowledged prejudice, can block the realization of what would appear to be the most attainable of goals —the election to power of majority-party candidates.

FIGURE 1

Political Participation for American Ethnic Groups (deviations from the mean).

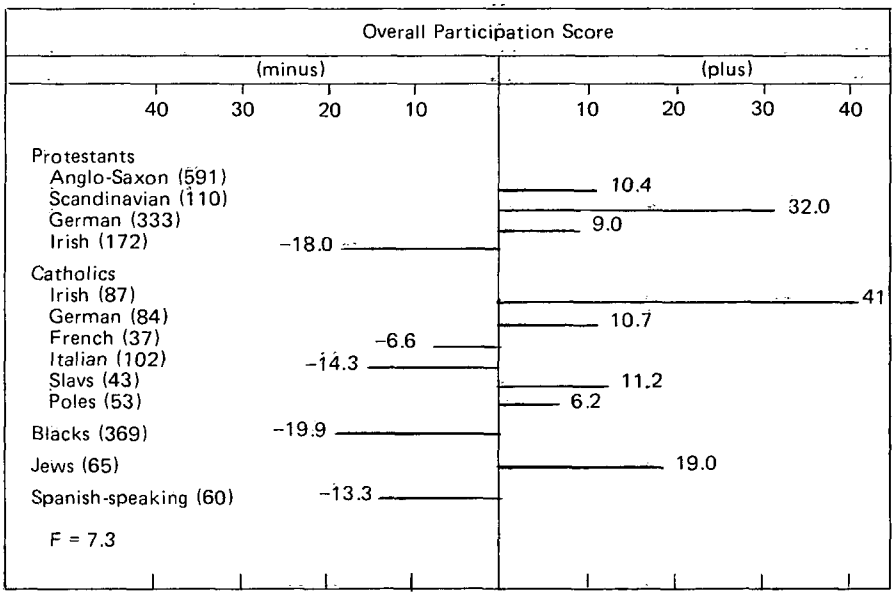

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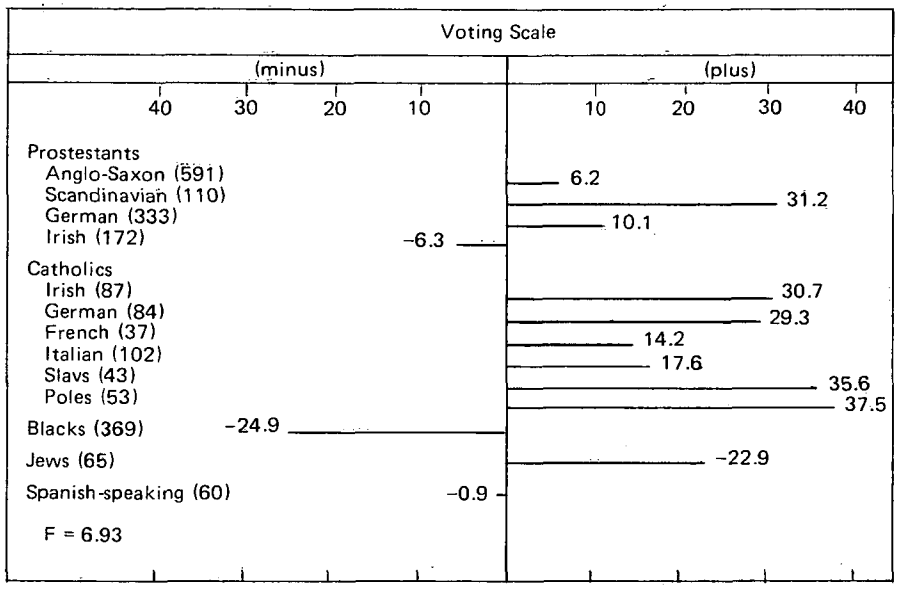
FIGURE 2

Political Participation for American Ethnic Groups (deviations from the mean).



Political scientists are interested in the whole range of political behaviors. Verba and Nie have developed a "political participation factor"[12] composed of four component factors: voting, political activism (campaigning, contributions, etc.), communal participation (membership in civic organizations), and "particularized contact" (writing letters to or personally contacting political leaders). (See Figures 1-5.) Irish Catholics are the highest on the political participation scale and also on the activism scale; they tie with Scandinavian Protestants on the communal participation scale. Polish Catholics are the most likely to vote, followed by the Slavic Catholics, then by the German, Irish, and Italian Catholics, with the Protestant groups trailing behind them. In addition, the eastern and southern European groups are the most likely to engage in particularized contact, with the Italians, Slavs, and Poles being ahead of Irish and German Catholics and all Protestants in their propensity to this kind of political behavior. (Perhaps it is the sort of behavior that occurs in neighborhoods where the precinct captains and other minor political figures are relatively accessible.) When we hold social class background constant, these patterns of political participation do not change.

So some Catholic groups are more active than others; indeed, the Irish Cath-

[12] Sidney Verba and Norman H. Nie, *Participation in America: Political Democracy and Social Equality* (New York, 1972).

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FIGURE 3

Political Participation for American Ethnic Groups (deviations from the mean).

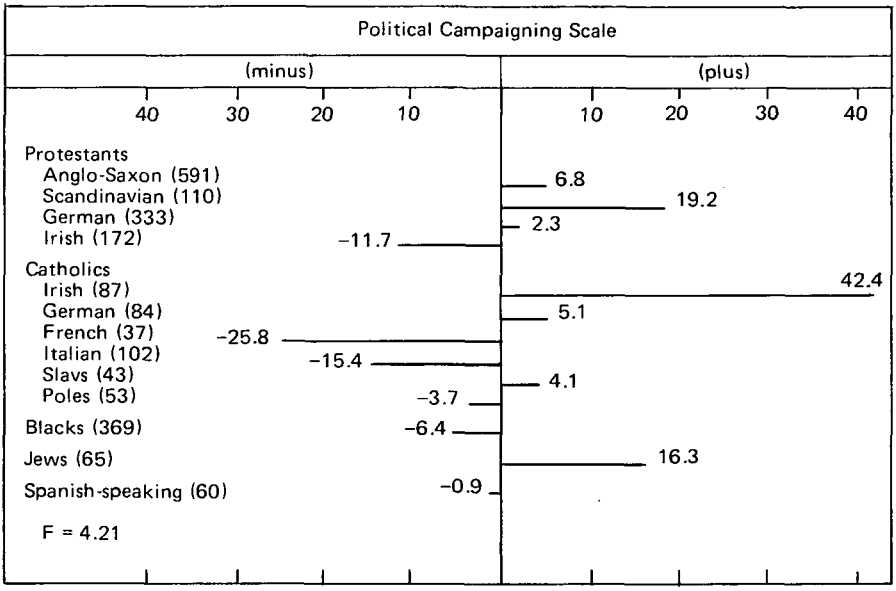

FIGURE 4

Political Participation for American Ethnic Groups (deviations from the mean).

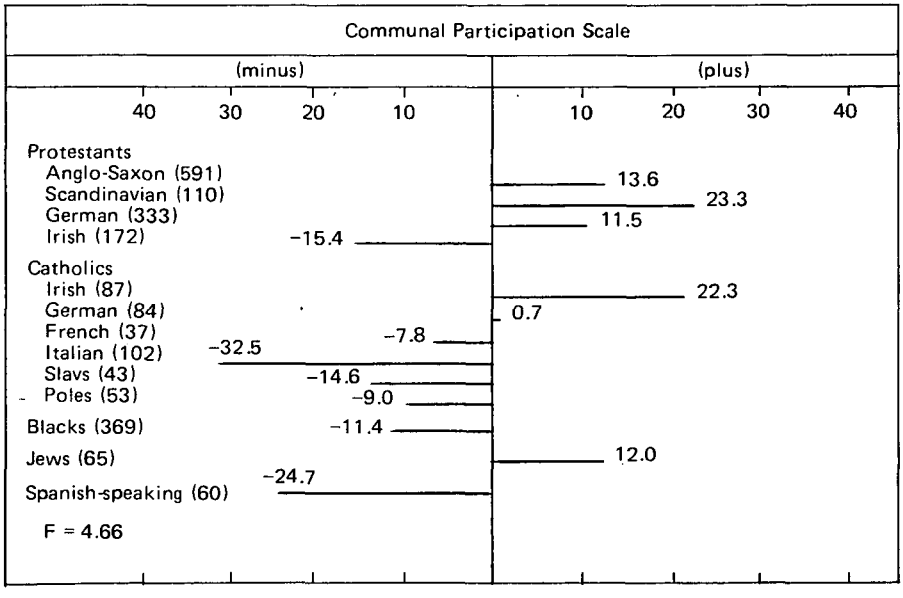

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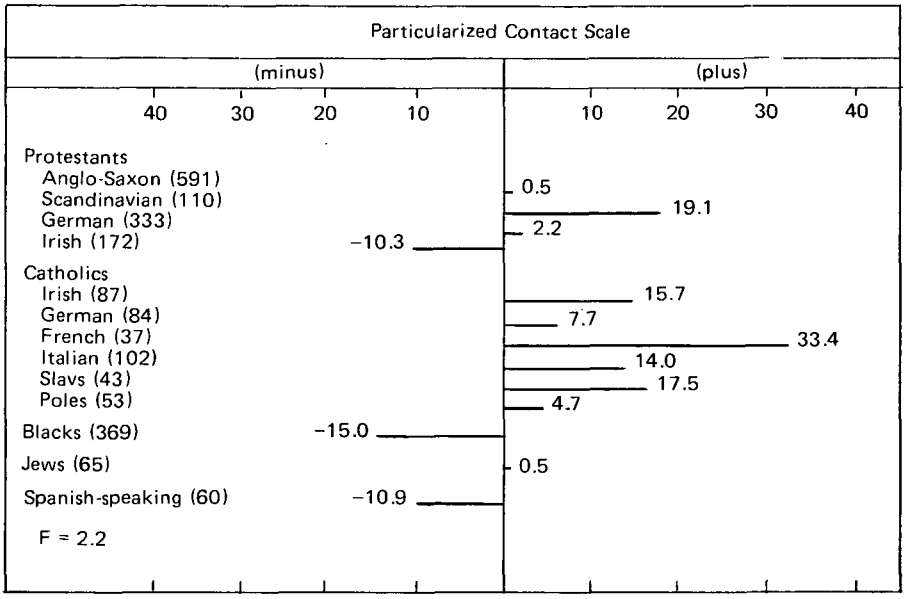
FIGURE 5

Political Participation for American Ethnic Groups (deviations from the mean).



olics are the highest political participators in the country. But all Catholics have one dimension of political participation at which they are the most active: Poles in voting; and Poles, Italians, and other eastern Europeans in particularized contact. Protestants lead in political participation of the communal or civic variety. Jews are also high on this form of participation, while Irish Catholics are the only Catholic group to score above the mean in civic involvement. Jews, for their part, are political activists and strong voters, but they are not much given to particularized contact. One sees the possibility for considerable misunderstanding of political styles across religious lines. The civic mode of participation is highly valued by Jews and Protestants but does not seem to attract Catholic groups besides the Irish. Particularized contact, however, is very popular with the eastern and southern European ethnic groups and not unpopular with the Irish, but it is not valued highly by either Jews or Protestants (except for German Protestants). Catholic groups (again excepting the Irish) seem to feel that they discharge their political responsibilities by voting and by particularized contact. Jews seem to feel that in addition to voting, one must also be involved in civic and campaigning activities. Protestants heavily emphasize civic participation. Responsible citizenship, then, means something different for Jews and Protestants than it does for eastern and southern European Catholics. The Italian and the Pole cast their votes, and when there is some kind of a problem, they go to their precinct captains. When the Protestant is faced with a problem, he joins a civic organization, and the Jew either joins a civic organi-

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zation or engages in campaign activism. The Irish Catholic (hedging his bets, perhaps) engages in all four varieties of political participation. We have here, perhaps, a classic manifestation of the differences of ethnic subcultures which persists unaffected by education and generation in America. Unless one understands tht another group has a different political style and respects the right to that style (without necessarily being persuaded that it is the best style), one has the raw materials for both misunderstanding and conflict. Joining a civic group is not in itself either superior to or more effective than calling your precinct captain; nor is it necessarily inferior or less effective; it is simply different.

It may well be, however, that differences in political style are more important than differences of political substance. The misunderstanding and misperception of Catholic voting, political affiliation, and political attitudes may be in fact not based so much on substantive differences between Catholics and other groups which are disproportionately represented in the Democratic party coalition as on subtle and unrecognized but powerfully important differences in the way in which one approaches political participation.[13] Catholics are not more conservative than other Americans, and they are certainly not more likely to have been hawkish on the Vietnam war or hard-hat on the social issues. But they are more likely to call their precinct captain and less likely to join civic organizations, and that may be the real problem.

[13] Despite polls up to the Friday before the 1976 election showing that Jimmy Carter only had 45 percent of the Catholic voters in the country committed to him, it would seem that over the weekend he picked up virtually all the uncommitted Catholics, and in fact, gained 56 percent of the Catholic vote, including 65 percent of the blue-collar Catholic vote. Despite considerable uncertainty and hesitation about Carter among Catholics, he still did substantially better than George McGovern (48 percent) though not as well as Hubert Humphrey (62 percent). It would appear than in an election in which neither candidate appeared terribly attractive to Catholic voters, the Democratic party preferences and past voting behavior was of decisive last minute importance to the undecided Catholics.

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Monitoring the Block Grant Program for Community Development

RICHARD P. NATHAN
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By and large, the social science community has not devoted much effort to implementation studies of major new federal programs. Longitudinal research on what happens to new laws once they are enacted requires not only a major investment of time, but, for a nation as large as the United States, a network of researchers equipped to tackle the multiple aspects of program impact. Once a new program is enacted, the tendency among social scientists has been to turn their attention to the next hot issue, rather than to conduct research on how well any given piece of policy fulfills its intended purposes.

This article reports on the highlights of an ongoing monitoring study by the Brookings Institution of the new block grant program for community development (CDBG). The Housing and Community Development Act which established this program was enacted August 22, 1974, as Public Law 93-383. The act authorized $8.4 billion to be distributed by the Department of Housing and Urban Development in the form of broad, flexible payments made by the federal government to qualifying local governments. The CDBG program took seven previ-

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ously established federal assistance programs—urban renewal, model cities, water and sewer facilities, open spaces, neighborhood facilities, rehabilitation loans, and public facility loans—and combined them into a single new grant program.

Framework of the Law

Although CDBG funds are less restrictive as to their use than were the individual program funds, there are legislatively established substantive and procedural requirements. An application is required from each participating city and urban county,[1] including so-called entitlement cities which receive a formula allocation under the new system. Each application must contain a three-year community development plan, an annual community development program statement, and what the act calls a "Housing Assistance Plan" (HAP). "National goals" and permissible uses for funds, although quite broad, are outlined in the act. Public hearings and citizen participation are required. Priority must be given to activities that benefit families with low or moderate incomes.

Within this framework, an application can be disapproved by HUD only if the intended uses are, in the language of the act, "plainly inappropriate" to meet the stated needs and objectives or if the facts presented or the stated needs and objectives are "plainly inconsistent" with the known facts. If such disapproval is not made within seventy-five days of submission, the application is automatically approved.

The Monitoring Study

The Brookings monitoring study, being conducted under a HUD research contract, contemplates two rounds of field research, each covering one year of experience under the new program. There are sixty-two jurisdictions in the sample, selected as a representative group of thirty central cities, twelve "satellite" metropolitan cities (not central cities) with populations above 50,000, ten urban counties, and ten nonmetropolitan jurisdictions. The sample represents 8.1 percent of all central cities in the program, 7.9 percent of all "satellite" cities, and 13.7 percent of urban counties. Sample jurisdictions are evenly distributed on a regional basis. They range in population from 9600 (Alma, Mich.) to New York City. In dollar terms, the sample accounts for 22.7 percent of all CDBG funds allocated in the first year of the program.

The study, like the parallel Brookings monitoring study of the general revenue

[1] An urban county is a county within an SMSA that (1) is authorized under state law to undertake community development and housing assistance activities in its unincorporated areas, and (2) has a total population of 200,000 or more, excluding the population of metropolitan cities within its borders and communities that specifically choose to be excluded from the urban county for CDBG purposes.

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sharing program,[2] covers three main areas: (1) the uses and beneficiaries of CDBG funds; (2) the allocation of CDBG funds by generalist officials of local governments; and (3) the impact of the federal distribution formula.

First-Year Findings

The following points summarize the findings for the first year of the Brookings monitoring study of the CDBG.

1. The basic idea of the block grant appears to be working to the extent that its proponents sought to combine two principal elements—an emphasis on capital expenditures for community development and features to promote greater flexibility for recipient governments in determining their communities' needs and priorities.
2. Capital spending clearly predominated in the first-year allocations of CDBG funds. This involved a combination of new capital spending (in large measure for relatively short-term purposes) and the continuation of preexisting and long-range urban renewal projects.
3. Attempts were made by many jurisdictions to use block grant funds as leverage to secure private funds as well as other public funds. The leveraging effects that can be observed, however, are not large and are characterized by implementation delays. Housing rehabilitation is the largest functional area for combined public-private activities undertaken with CDBG funds.
4. Generalist officials, particularly chief executives, have played prominent roles in community development decision making, both on procedural and substantive matters. Their roles tend to be much more important under the block grant program than they were under the preexisting federal grant programs for community development, which in the case of urban renewal and model cities generally were administered by quasi-autonomous or independent local agencies or authorities.
5. Citizen participation, contrary to what some observers anticipated, has been a very significant feature of the program's implementation in the first year, especially in those cases in which local officials demonstrated a strong positive attitude toward these activities.
6. The CDBG program did not significantly contribute to the legislative objective of encouraging "spatial deconcentration" of housing for lower-income persons. In almost all of the sample sites, associates reported that local officials did not consider this objective in preparing their applications, and furthermore that the related requirements for a Housing Assistance Plan were not emphasized or consistently enforced.
7. A similar situation was found to apply to the "A-95" requirement for metropolitan planning and coordination, a clearance system closely related (or at

[2] The second volume on the Brookings monitor study on the general revenue sharing program has just been published: Richard P. Nathan, Charles F. Adams, Jr., and Associates, *Revenue Sharing: The Second Round* (Washington, D. C., 1977).

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least potentially so) to the "spatial deconcentration" objective in the law. *Pro forma* sign offs predominated under the A-95 requirement.

8. In relative terms the allocation system favors small and suburban jurisdictions; it disadvantages older and distressed central cities.

While our study generally avoids recommendations for program changes, we do suggest specific changes in the allocation system and show how they would affect major units entitled to CDBG funds. Two major types of changes, discussed later, are considered, a "dual formula" for the basic allocation system and supplemental CDBG grants, or "bonus" grants, for needy cities.

The Uses and Beneficiaries of CDBG Funds

Beyond the broad goals of developing "viable urban communities . . . , and a suitable environment . . . , principally for persons of low and moderate income . . . ," the Housing and Community Development Act of 1974 mandates no single most desirable usage of community development block grant funds, nor does it specify what combination of activities the funds should support. Within the broad parameters of the act, applicants and recipients have latitude to determine and implement their own programs for community development.

At this point, we can look at some specific examples of the impacts of the grants, first in two large cities.

In *Boston, Massachusetts* (population 641,000), the focus of the first year of the CDBG program was on neighborhood stabilization. The city received $32.1 million. Boston has high unemployment, a high property tax rate, and a recently reduced borrowing rating; it also faces a projected loss under the full CDBG formula. The city allocated a large portion of its grant to capital projects outside the former model cities and urban renewal areas. According to the field research associate, "There was an explicit desire on the part of the mayor's office to provide some visible artifact in each neighborhood of the allocation of these funds—street lights, parks, sidewalks, community facilities, trees." A spreading effect is reflected not only in spending for public facilities and improvements, which totaled about $6 million, but also for the $7.25 million housing rehabilitation program. Housing rehabilitation was carried out in neighborhoods throughout the city. Boston utilized a citizen participation strategy for CDBG focused on its eighteen "Little City Halls." The associate reported that in a mayoral election year this spreading effect for benefits could have been predicted. According to the associate, "The block grant budget presented an opportunity for the mayor to exercise his discretion and to distribute money his way rather than HUD's."

Pittsburgh, Pennsylvania (population 520,089), which received $16.4 million under CDBG in 1975, had extensive experience under the previous "categorical" grants, particularly in the urban renewal and model cities programs. Even before the advent of the block grant program, the city was in the process of phasing out these two programs, which officials viewed as "too expensive." According to the field research associate, city leaders, strongly committed to fiscal conservatism and the maintenance of a budgetary surplus, showed "a willingness to undertake

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only those programs of a capital nature that can be completed within a short period of time, and impose only the minimal and necessary operating cost to the city." The emphasis was on capital improvements rather than new facilities. With CDBG funds, improvements were made to existing recreational centers, playgrounds, swimming pools, and senior citizens' centers. No new facilities that could lead either to a long-term financial commitment or to continuing operating costs were included in the block grant applications. No new operations were undertaken with block grant funds. The underlying concern in the allocation of funds was twofold: (1) to avoid expanding programs beyond what could be accomplished entirely with CDBG funds so that, in the event the federal grant was suddenly withdrawn, the city would not be burdened with the cost of such programs; and (2) to prevent the further decline of good residential areas which were excluded under the old categorical grant programs.

Urban counties also receive grants under the block grant program for community development. In *Orange County, California* (population 1,420,386), the block grant had an especially important impact.

The Orange County administration had in recent years sought a more prominent role in coordinating the development activities of both incorporated cities and unincorporated areas. The CDBG program was found to have contributed to an increased role for county government. The county's administrative staff, especially the planning department, had been increasingly concerned about growth problems and limited opportunities to influence how incorporated cities respond to these problems. In the view of the field research associate, the CDBG program "for the first time gave county planners the opportunity to have leverage over the cities to force integrated planning and attention to the needs of the county's low- and moderate-income population." The county received $1.4 million for 1975.

Although Orange County qualified as an urban county with only the population of its unincorporated areas, the county government encouraged participation by the incorporated cities in a joint application. Responsibility for negotiating with the cities and preparing the county's application was assigned to the county planning department. A staff team from the department spent many weeks explaining the program to city officials and negotiating for their participation. The associate reported that "initially, many of the cities were reluctant to get involved in an application for federal funds, particularly HUD funds, which connote such things as 'integrated housing' and 'economic mixing.'" Ultimately, however, all seventeen cities with populations under 50,000 joined the county in the first-year application.

For its part, the county promised that in addition to passing on funds to the participating cities, it would undertake its own projects in unincorporated areas adjacent to participating cities. "It provided technical assistance to the cities needing it, and generally encouraged expenditures on activities likely to benefit low- and moderate-income groups."

Small cities, too, can receive block grants for community development, on a discretionary basis. For such units, the new program tends to operate in much the same manner as the earlier categorical programs. A good illustration of this is *Casa Grande, Arizona* (population 11,000).

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Casa Grande's application for discretionary funds (it received $235,000 in 1975) was tailored to meet HUD area office demands. Although the city had hoped to be awarded aid for property acquisition and parks, the application was modified to include only capital spending for housing rehabilitation, which accounted for three-fourths of the first-year grant. According to the field research associate, "For all intents and purposes, the finally approved version of the application was perceived by the local officials to be categorical. The funds were new, to be spent for a limited purpose in accordance with federal directives and regulations. Although forms and procedures may be different, I can't see any meaningful difference between the process Casa Grande has gone through and any other categorical grant request. I know that it is the way local officials perceived the process." Although local officials would have preferred to share the benefits of this grant with all residents in need of housing assistance, HUD required the selection of a specific target area.

Under any law, the impact of federal funds on recipient governments is a central problem in the analysis of a grant program. Under a broad-gauged program, it is especially complex. Several questions necessarily arise. What differences do CDBG funds make in the expenditure and revenue decisions of recipient jurisdictions? How does federal assistance affect particular functions and programs? What is its impact on the economy of recipient communities and on different income groups?

Fiscal and Programmatic Effects

In analyzing fiscal effects, the field research associates were asked to answer a difficult question: What did CDBG funds enable recipient governments to do that they would not otherwise have done? An important aim of this analysis has been to identify how and to what extent CDBG funds have been used to pay for activities that would otherwise have been undertaken and paid for from other revenue sources. These so-called substitution effects occur under all types of fiscal subventions. The associates classified fiscal effects as new spending, program maintenance, and substitution effects, and the three categories were subdivided and defined as follows:

1. New spending effects

   a. New capital spending. Spending for capital projects or the purchase of equipment that, without block grant funds, would either not have occurred at all or would have occurred at least one year later.

   b. New or expanded operations. Operating expenditures initiated or expanded with block grant funds.

2. Program maintenance effects

   a. Community development program continuation. Allocating CDBG funds to ongoing programs formerly funded by grants now included in the CDBG program.

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b. Continuation of noncommunity development federally aided programs. Allocating CDBG funds to ongoing programs formerly funded by grants not related to community development that, without block grant funds, would now be cut back or eliminated.

c. Other program maintenances. Allocating CDBG funds to ongoing programs formerly not federally assisted which without CDBG funds would now be cut back or eliminated.

3. Substitution effects

a. Increased fund balances. Allocating CDBG funds to ongoing programs, with the net effect of increasing fund balances.

b. Avoidance of borrowing. Substituting CDBG funds for borrowing that would have been undertaken in the absence of CDBG funds.

c. Tax stabilization. Using CDBG funds to finance existing programs, in order to avoid a tax increase that otherwise would have been necessary and approved.

d. Tax reduction. Using block grant funds to finance ongoing programs, in order to keep up the jurisdiction's own resources and thereby permit a reduction in the tax rate.

In contrast to general revenue sharing, new spending and program maintenance are far more prominent fiscal effects of the block grant program than are substitution effects. For the sample as a whole (sixty jurisdictions are included here), more than 53 percent of the first-year allocations were for new spending purposes, almost 32 percent were for program maintenance, and only 7 percent were for substitution effects. New capital spending and community development program continuation are the two largest fiscal effects for all types of recipients as shown in Table 1.

New capital spending was the biggest single net effect. In the fifty-five sample units that allocated funds for new capital purposes, spending for public facilities (for example, centers for health, recreation, and community activities) and for housing rehabilitation (both loans and grants) accounted for two-thirds of total new capital allocations. Fifteen percent was devoted to spending for public improvements (streets, sidewalks, and sewers) and 12 percent for property acquisition.

Looking at the new capital spending data in the aggregate, it is necessary to ask whether and to what extent these uses resemble the kinds of capital projects undertaken by the previous programs. Two general observations can be made. First, a focus on conservation and rehabilitation, rather than on land acquisition and new construction, emerges. Second, the location of these new capital uses indicates a distinct shift away from the typically more concentrated geographical pattern of the urban renewal program. These two findings are interrelated. The net result is that new capital spending under the CDBG programs tends to have a geographical *spreading effect*, particularly evident in plans for "neighborhood conservation programs."

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TABLE 1

Net Fiscal Effects of CDBG Allocations in the Sample Jurisdictions, by Type of Recipients;[a] in Mean Percentage Indices[b]

Fiscal Effect	Metropolitan Jurisdictions			Nonmetropolitan Jurisdictions	All Jurisdictions
	Central Cities	Satellite Cities	Urban Counties		
New spending	49.1%	52.9%	44.9%	74.0%	53.3%
New capital	34.8	33.3	27.2	57.9	37.1
New or expanded operations	14.3	19.6	17.7	16.1	16.2
Program maintenance	44.0	25.1	17.3	19.3	31.9
Community development program continuation	42.1	11.3	16.3	17.8	27.9
Noncommunity development federally aided program continuation	1.1	.2	1.0	c	.7
Other program continuation	.8	13.6	0.0	1.5	3.3
Substitution	3.4	16.7	5.9	4.3	6.6
Increased fund balances	2.4	5.3	2.5	0.0	2.6
Avoidance of borrowing	.7	10.0	3.1	4.2	3.5
Tax stabilization	.3	1.5	.3	.1	.5
Tax reduction	c	0.0	0.0	0.0	c
Other	c	0.0	1.5	0.0	.3
Unallocated	3.5	5.3	30.4	2.4	7.9

SOURCE: Field research data.

[a] For purposes of this table, the sample includes twenty-nine central cities, twelve satellite cities, ten urban counties, and nine nonmetropolitan jurisdictions. Two jurisdictions in the sample (Florence and Columbia, S. C.) are not represented because data from them were not available at the time this table was prepared.

[b] The mean percentage indices of fiscal effects are unweighted; that is, they are not weighted to reflect different population sizes of sample units.

[c] Mean percent index less than .1.

Income-Group Effects

There is a second and more difficult question for analysts of government programs: *Who benefits?* The block grant title of the Housing and Community Development Act of 1974 has something to say about who should benefit. It states that the program should give "maximum feasible priority to activities which will benefit low and moderate-income families or aid in the prevention of slums and blight." This wording, including the qualifying second phrase, however, is less precise than the provision in the Senate bill, removed in conference, which would have limited to 20 percent the proportion of program expenditures "not intended to be of direct and significant benefits to families of low and moderate income or areas which are blighted or deteriorating." Despite the language of the

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act calling for priority to expenditures on lower-income groups, CDBG applications may in fact be approved if the applicant certifies that "other activities (aside from those targeted on low- and moderate-income families) are required to meet other community development needs having a particular urgency." There is, then, a general intent of Congress concerning low- and moderate-income groups, but in keeping with the flexibility goal of the block grant program, the legislation is neither specific nor mandatory on this point.

Methodological problems in analyzing the income group benefits involved definitions of lower- and moderate-income persons and assessing the nature of benefits to them. Low income is defined as less than 50 percent of the median income of the SMSA; moderate income is 50–80 percent. A relatively narrow concept of benefits was adopted, concentrating on *direct and immediate or near-term* benefits. Table 2 presents first-year and tentative data on the impact of the uses of CDBG funds on different income groups within the sample jurisdictions, as assessed by the field research associates. For the sample cities, low- and moderate-income groups emerged as the beneficiaries of a majority of the first-year CDBG allocations on an unweighted mean basis. Almost 52 percent of all program benefits were assigned to these two groups, as shown in Table 2.

The figure in Table 2 of 28.7 percent of direct program benefits to low-income families and individuals, of course, needs to be considered in the context of the categorical grants. Of these earlier amounts, 25.4 percent was allocated for the

TABLE 2

Income-group Incidence Data for Fifty Sample Jurisdictions by Unweighted Mean Percentage

Income-group Categories	*Mean Percentage*
Low-income families and individuals	28.7
Moderate-income families and individuals	23.2
Other income groups	
Communitywide/nonincome specific	25.4
Not allocable on this basis	12.8
Number of jurisdictions	50[a]

SOURCE: Field research data.

[a] Central cities, satellite cities, and nonmetropolitan jurisdictions only. Urban counties are excluded; income-incidence data were not available for Columbia and Florence, S.C., when this table was prepared. Although neighborhoods (usually census tracts) were used for this analysis, associates in some cases identified projects as directly benefiting lower-income persons even though they were not in high-concentration poverty areas. If half or more of the benefits were for lower- or moderate-income persons, a project was included in whichever group applied. In most cases, the 50 percent rule was not needed; projects for lower- and moderate-income persons were mainly for their benefit. To emphasize that this is a difficult assessment task, associates were asked to summarize the income- group incidence findings in 5 percentage point intervals. In combining the data, however, we have used more specific summary percentages in this table.

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model cities program, 64.9 percent for urban renewal (which sometimes did and often did not benefit the poor), and 9.7 percent for the other programs.

These on-the-scene benefit assessments by our associates are, to restate, direct near-term benefits. The point is often made—and often defensible—that secondary and perhaps longer-term benefits to lower-income groups are of major importance. In many ways, this is the essential question for analysis. In a number of communities there was open debate about the merits of direct investments in the poorest areas versus what are considered transitional or salvageable neighborhoods. In a long-term context, these transitional neighborhoods may be the areas into which the poor might move in the future.

It is in a way reassuring that a similar debate has been conducted at the national level and in the academic literature: that is, should the nation's strategy for dealing with the often poorly defined "urban crisis" emphasize the physical and social development of the most seriously distressed urban areas or concentrate instead on neighborhood conservation? In keeping with the decentralization purposes of CDBG, this debate can be said to be shifting from the national to the local level.

Although low- and moderate-income groups are the main direct beneficiaries of first-year allocations, they do not benefit uniformly among all types of jurisdictions. The average proportions of benefits were 60 percent for the low- and moderate-income groups in central cities, 53 percent in nonmetropolitan units, and 29 percent in satellite cities. Three factors appear to explain these variations: (1) the size of the poverty population, (2) a jurisdiction's experience under the model cities program, and (3) the nature and effectiveness of citizen groups. Thus we can make the following general conclusions:

1. The higher the poverty rate of a given jurisdiction, the higher the proportion of direct program benefits allocated for lower-income persons
2. Jursidictions with model cities experience allocated higher proportions of direct program benefits to low- and moderate-income groups than those without model cities experience
3. Where effective citizens' groups are organized around demands relating to low- and moderate-income concerns and where public officials are sympathetic to these demands, benefits tended to be higher than in communities where this is not the case

Overall Impact

Putting together the data on uses and net effects with that on income incidence, it is possible to generalize about strategies and patterns in the allocation of first-year CDBG funds. *Far and away the predominant approach to community development under the block grant program in its first year of operation involved a neighborhood conservation and growth strategy designed primarily to prevent urban blight.* Several groups of sample units come under this approach. We distinguish between old and new target areas; the former refers to areas which were

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targets for the model cities program or for urban renewal, the latter to lower-income areas which were not included under the categorical programs.

Of the fifty cases in Table 2, there were none which we classified as having a decidedly redistributive pattern in their use of CDBG funds—that is, of involving a shift away from lower-income target areas under the categorical programs to moderate- or upper-income areas. As stipulated in the law, the plans for the sample units tended to emphasize low- and moderate-income groups and areas; the main exceptions were communitywide development projects, which in many cases were justified because their principal long-term beneficiaries would be low- and moderate-income persons. In only three cases (Auburn and Bangor, Maine, and Sioux City, Iowa) did recipient jurisdictions use most of their CDBG funds to complete ongoing downtown renewal projects. These cases come the closest to being redistributive.

> *Auburn, Maine* (population 24,151), for example, had no model cities experience and a poverty population of 10.9 percent; it allocated 80 percent of its first-year block grant to complete its downtown urban renewal project. A citizen advisory committee indicated interest in a new inner-city development project along with citywide public improvements. However, the city manager and a city planner decided instead, partly as a result of conversations with the HUD area office, that it was in the city's best interest to complete the Great Falls urban renewal project. Fear of jeopardizing prior city investments and of possible legal complications concerning the payment of urban renewal notes were major factors in reaching this decision. Alternative funding choices initially proposed by the citizen advisory committee for the CDBG program were not explored once the decision to continue the urban renewal project had been made.

Twenty-seven sample cities were classified as having a defined neighborhood conservation and growth strategy, concentrating on identified target areas, as shown in Table 3. In seven of these cases, the bulk of first-year CDBG funds was

TABLE 3

Strategies and Patterns in the First-year Use of CDBG Funds for Fifty Cities, Brookings Monitoring Study, January 1977

	Numbers of Jurisdictions Strategy of Community Development			
Geography of Allocations	*Completion of Urban Renewal*	*Defined Neighborhood Conservation*	*Generalized Neighborhood Conservation*	*Citywide*
Existing target areas only	3	–	–	–
Existing and new target areas	–	20	13	–
New target areas only	–	7	1	–
No target areas	–	–	–	6

SOURCE: Field research data.

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allocated for old target areas. In the rest, new target areas received more than half of the first-year allocations.

St. Louis was one of the cities classified as having a defined neighborhood conservation and growth strategy in which a majority of first-year CDBG allocations were made in old target areas. According to the associate, the rest was used in relation to "a new housing strategy to save what could be preserved, revive some areas, and in others eliminate deteriorated housing."

Among the cities with a defined neighborhood strategy which allocated more than half of their CDBG funds in the first year for new target areas, two examples are of particular interest. For Cleveland, the associate specifically mentioned a "triage philosophy" focused on "salvageable" neighborhoods. In Houston, the associate reported that the framers of the first-year application envisioned that CDBG funds for housing rehabilitation would be supplemented in the target neighborhoods by locally generated funds earmarked for neighborhood public improvements—streets, lighting, and sewerage.

Fourteen cities were classified in the first year as having adopted a generalized, as opposed to a defined, neighborhood approach. Low- and moderate-income neighborhoods were stressed, but the definition of the areas involved tended to be less clear. For example, in Denver, it was noted by the associate that all council districts received some CDBG funds. Most funds were spent in the model cities area, "the rest on a concentrated basis in selected neighborhoods throughout the city." Likewise, in Newark, New Jersey, the associate noted "a wider distribution" of funds under CDBG with supportive service programs "geared for citywide impact." Philadelphia is also in this group. Here the associate reported that CDBG funds were used "citywide, but on a neighborhood-centered basis." In a similar vein, the associate for Huntington Beach, California, reported that there was a "concentration of projects in low- and moderate-income areas," along with projects for public facilities with citywide benefits.

Six sample units, all of them relatively small, were classified as having used the bulk of their CDBG funds on a citywide basis. Two are nonmetropolitan discretionary units. The remaining four are satellite cities eligible for formula allocations. In the latter group, for example, Greece, New York, used its CDBG funds for drainage projects. Cleveland Heights, according to the associate, specifically avoided the designation of a target area, believing that "such a designation would result in a negative label being applied to the area involved."

As these various strategies come into clearer view, they suggest important questions for the second stage of the research; current plans call for the collection of additional field data on how target areas are defined and selected, as well as data on who benefits from these selections.

The Allocations of CDBG Funds by Generalist Officials of Local Governments

Federal grant programs are more than fund transfers. They are, in varying de-

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grees, transfers of power. While the political consequences of grant programs have long been apparent, they are often not taken into account in designing federal grants-in-aid. In the CDBG program, however, they were explicitly considered. The program was deliberately designed to bring about certain political consequences. These are usually referred to under the designation, "decentralization," involving the participation of both generalist officials of local government and private citizens. Who, then, decides on the allocation of these funds?

First, local government officials have become more influential in policy making for community development than was the case prior to CDBG. Local chief executives and other generalist officials of the executive branch have been the principal actors. This applies both to the procedural and substantive aspects of the CDBG program. There was significant involvement by local legislatures in nearly half of the sample units in the first year of the CDBG program.

Second, the role of specialists was commensurately downgraded, with some specialized urban renewal and model cities agencies playing no role at all. In a few cases, where staff members of specialized agencies became key CDBG decision makers, they were absorbed into other agencies of city government.

Third, the CDBG program has prompted administrative reorganization which in a number of cases has been conducive to more comprehensive planning and policy development at the local level. Planners generally have benefited in this context.

Fourth, by placing general-purpose local government in charge of decision making, the block grant program has stimulated a more competitive policy-making process in many jurisdictions. This increased competition, involving chief executives, legislators, and specialists, becomes even more apparent when we take into account the involvement of various citizens' organizations. In this connection, one should note that benefits which may result from comprehensive policy making may be dissipated by the more competitive political process under the block grant programs.

Fifth, local governments went well beyond the minimum legal requirements in encouraging and providing for citizen participation. The field data indicate that the level of citizen participation was substantial in most sample jurisdictions. Minority groups (both blacks and Hispanic) were heavily represented. Another important characteristic of the element of citizen participation was its tendency to be on a broader basis in the community, involving more groups and areas, as compared to the old target areas, for example, under the model cities program. This can be seen as *a political spreading effect*, parallel to the spreading of the programmatic effects and benefits noted earlier.

Impact of Citizen Participation

The fifth point, above, leads directly to the issue of whether, and in what way, citizen participation influenced the decisions and policies reflected in applications for CDBG funds. In the first-year CDBG application process, citizens made

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their views known in various ways—some of them formal and public and thus available for examination by researchers, but others in an informal setting, making it impossible to identify or evaluate the impact of citizens on decision making. Moreover, what occurs in the citizen participation process is not a simple, one-way transmission of ideas from citizens to the appropriate officials; rather, it is a much more complex interaction in which local officials can in many ways shape the kinds of demands made by citizens.

With these points in view, we tried to ascertain how and to what extent citizen participation influenced the preparation of CDBG applications. In addition, we examined the impact of specific major participating organizations. To deal with the first issue, we relied on summary assessments made by the field associates after they had completed their reports on the first-year application process. They were asked whether the following statement applied: "Citizen participation was influential in terms of the final outcomes contained in the CDBG applications." The associates were instructed to rank their responses according to whether the statement applied in their jurisdictions without qualification, whether it applied to a limited extent, or whether it did not apply. In eighteen jurisdictions citizen participation was judged not to have been influential; in twenty-six jurisdictions it was judged influential to a limited extent; and in eighteen jurisdictions it was rated as influential.

For a closer look at the influence of citizen participation in the application process, the research focused on the eight major participating groups. Data for these groups according to the extent of their influence on the CDBG applications in the sample jurisdictions is shown in Table 4.

Three groups stand out by a significant margin as being most influential—neighborhood groups, senior citizens, and model cities organizations. The case of senior citizens is particularly interesting in that they were identified as among the most influential in twenty jurisdictions although they were among the most active in only twelve of these jurisdictions.

TABLE 4

Community Group Involvement in the CDBG Application Process

Organizations or Groups	*Jurisdictions Rated: "Among the Most Active"*	*Jurisdictions Rated: "Among the Most Influential"*
Neighborhood groups	36	28
Senior citizens' groups	32	20
Model cities groups	22	11
Housing groups	11	5
League of Women Voters	12	2
Civic organizations	15	2
Areawide/citywide minority organizations	21	–
Religious groups	6	–

SOURCE: Field research data.

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TABLE 5

Type of Citizen Participation Structure, by Extent of Citizen Influence[a]

Type of Citizen Participation Structure	*Extent of Citizen Influence*		
	Influential	*Moderately Influential*	*Not Influential*
Public hearings only	2	4	7
Public hearings and neighborhood meetings	10	5	2
Citizens' advisory committees and public hearings	1	11	7
Citizens' advisory committees, public hearings, and neighborhood meetings	5	5	2

SOURCE: Field research data.

[a]One jurisdiction, Hennepin County, did not offer citizen participation at the country level.

It is also worth noting that although citywide/areawide minority organizations ranked fourth among the most active groups, with an "active participation" rating in twenty-one jurisdictions, in no jurisdictions were they judged to be among the most influential.

A number of different organizational approaches were taken to citizen participation under the CDBG program. When we began the study, we expected to find citizen participation more influential in jurisdictions where citizens' advisory committees were used, because they were organized, generally experienced, and had the potential for continued working involvement. Table 5 shows, however, that this was not the case. The format that was associated most often with significant influence was public hearings plus neighborhood meetings. Ten of the nineteen cases in this category showed influential citizen participation. The lowest incidence of influential participation was among the thirteen jurisdictions with public hearings only. In five of these, citizen participation was influential or moderately influential. The experience of jurisdictions with citizens' advisory committees and public hearings was surprising: among the seventeen in this category, ten had moderately influential participation; in only one case—Minneapolis—was participation judged influential.

The factor that appeared to have been the strongest determinant of whether citizens effectively contributed to the CDBG application was the amount of importance attached to citizen participation by local officials. Table 6 shows that of the twenty-three jurisdictions where public officials were reported to have viewed such participation as being very important, fifteen had influential citizen participation while only one was not influential. (The table shows the associates' assessments of citizens' influence in relation to the importance assigned to citizen participation by local officials.)

The analysis of the field data, then, leads us to the conclusion that the attitude of local officials, and not so much the procedures set up for citizen participation, was the key factor in determining whether or not citizens' views were reflected significantly in the block grant application. This finding tends to support the

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TABLE 6

Local Officials' Views and the Influence of the Citizen Participation Process—Summary Statements

Importance of Citizen Participation in CDBG Application Process: Local Officials' Views	*Influence of Citizen Participation Process*			
	Not Influential	*Somewhat Influential*	*Influential*	*Total*
Not important	11	1	–	12
Moderately important	6	17	4	27
Very important	1	7	15	23
Total	18	25	19	62

SOURCE: Field research data.

view of the program's framers that a prescribed format for participation would not particularly strengthen the citizen participation process under the CDBG program. A second finding is that overall there was a lack of any clear relationship between the activity level of citizen participation and its influence. Of course, all of the findings and conclusions in this report pertain only to the first program year, and may change as the program progresses.

The Impact of the Federal Distribution Formula

Formulas, often at the center of domestic policy making, are designed to give preference—whether stated or unstated—to certain kinds of jurisdictions or conditions. Analytically, a distribution formula has two components: (1) the eligibility element—who may participate in the program; and (2) the allocation process—how much money goes to the eligibles. For the CDBG program, nearly all general-purpose units of state and local government (meaning any city, county, town, township, parish, or village) are eligible to compete for CDBG funds, but only a relatively small percentage are actually *entitled* to funding. There are two forms of entitlement.

1. A *formula entitlement* is a sum earmarked for a particular community on the basis of the three formula criteria: population, overcrowded housing, and poverty (weighted twice). This amount is not available to any other community unless the entitlement community does not use the money. Formula entitlements go to all central cities in metropolitan areas; certain eligible urban counties, towns, and townships; and all municipalities other than central cities that have populations of 50,000 or more.

2. A *hold-harmless entitlement* essentially guarantees communities as much funding during each of the first three years of the new program as the annual average of what they had received under the categorical grant programs during the five-year period from 1968–1972. After the third year, in all situations, the excess of hold-harmless amount over the formula entitlement is to be phased out by thirds. Thus, by the sixth year, all central cities, other metropolitan cities

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with populations over 50,000, and urban counties would receive their actual CDBG formula amounts. There would no longer be hold-harmless funds and all hold-harmless communities not entitled to formula funding would be eligible only for discretionary funds as discussed below.

The following was the breakdown of the 1342 jurisdictions with formula or hold-harmless entitlements for fiscal year 1975:

Metropolitan cities with formula entitlement	
Central cities	365
Noncentral cities	156
Urban counties	73
Hold-harmless communities	
Metropolitan	299
Nonmetropolitan	449

In addition to the formula and hold-harmless funds, the act also provides *discretionary grants* for distribution to smaller communities (population under 50,000) within metropolitan areas. These smaller communities compete against others within a given metropolitan area. Communities outside of metropolitan areas have a separate discretionary fund for which they can compete.

Comparing the New and the Old

A central question of the distributional analysis under the CDBG program concerns the relative advantage or disadvantage to communities under the block grant system as compared with the situation in previous years under the folded-in categorical grant-in-aid programs.

In the early years of the program the ultimate distributional effects of the block grant formula are obscured because of the operation of the hold-harmless provisions. The distributional implications of the CDBG program can be seen in their entirety by projecting to fiscal year 1980, when the formula would be fully implemented, assuming for the moment that the law is continued in its present form beyond 1977.

In this analysis, confined to the fifty states and the District of Columbia, comparisons are made between funding levels under the categorical programs, represented by the 1968–1972 hold-harmless base period, and a projected 1980 allocation. The projected funding level used for fiscal 1980 is $2.95 billion, the amount authorized in the original legislation for the CDBG program for fiscal years 1976 and 1977. Comparisons are made both in total dollar terms and on the basis of per capita indexes expressed as a percentage of the national mean.[3]

[3] For purposes of this analysis, the number and mix of eligible recipients is held constant. In reality, the number will increase with the addition of new central and satellite cities and urban counties.

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TABLE 7

Categorical or Folded-in Grant Distribution by Region Compared with Projected Formula Allocation of CDBG Funds in Fiscal 1980; Levels of Overcrowded Housing and Poverty

Region[a]	Folded-in (millions of dollars) (1)	Percent of Total (2)	Formula[b] (millions of dollars) (3)	Percent of Total (4)	Change in Share (percent) (5)	Folded-in per Capita Index (6)	Formula per Capita Index (7)	Per Capita Index Change (8)	OVH Index (9)	Poverty Index (10)
U. S. total	2088	100	2790	100	—	100	100	—	100	100
New England	206	9.9	130	4.7	−5.2	170	80	−90	84	65
Middle Atlantic	475	22.7	486	17.4	−5.3	124	95	−29	84	77
East North Central	332	15.9	480	17.2	+1.3	80	87	+7	88	72
West North Central	161	7.7	188	6.7	−1.0	96	84	+12	92	98
South Atlantic	314	15.0	460	16.5	+1.5	99	109	+10	111	129
East South Central	125	6.0	221	7.9	+1.9	95	126	+31	124	178
West South Central	170	8.2	345	12.4	+4.2	86	130	+44	135	155
Mountain	75	3.6	110	3.9	+.3	88	96	+8	113	101
Pacific	230	11.0	370	13.3	+2.3	84	101	+17	98	80

SOURCES: The folded-in grant allocations and the indices were calculated directly from hold-harmless determinations and data elements used by the Department of Housing and Urban Development in making first-year CDBG allocations. The fiscal 1980 allocations were computed by applying the first-year data on objective needs to a projected fiscal 1980 appropriation.

[a] Regional divisions used by the U. S. Bureau of the Census are: *New England*—Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, Connecticut; *Middle Atlantic*—New York, New Jersey, Pennsylvania: *East North Central*—Ohio, Indiana, Illinois, Michigan, Wisconsin; *West North Central*—Minnesota, Iowa, Missouri, North Dakota, South Dakota, Nebraska, Kansas: *South Atlantic*—Delaware, Maryland, the District of Columbia, Virginia, West Virginia, North Carolina, South Carolina, Georgia, Florida: *East South Central*—Kentucky, Tennessee, Alabama, Mississippi; *West South Central*—Arkansas, Louisiana, Oklahoma, Texas: *Mountain*—Montana, Idaho, Wyoming, Colorado, New Mexico, Arizona, Utah, Nevada; *Pacific*—Washington, Oregon, California, Alaska, Hawaii.

[b] The SMSA discretionary fund allocations, incorporated into the formula amounts, are approximations. They do not take into account the population, poverty, and overcrowded housing of hold-harmless communities that will be added to SMSA discretionary areas. Thus the allocated discretionary amounts may change somewhat. The allocated discretionary dollar figures are based on data balances for years 1 to 3.

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Regional Patterns

Regionally, the New England and Middle Atlantic states were most "advantaged" in the distribution of funds under the previous programs. In fact, these two areas were the only ones in which per capita grants under the categorical programs exceeded the national average of communities receiving funds. The per capita grant in New England was 70 percent above the national mean; the Pacific region, by contrast, had a per capita grant 16 percent below the national average. (See column 6 of Table 7.)

Under full formula funding the most significant gains are made by the sixteen states of the three southern regions—South Atlantic, East South Central, and West South Central. Their per capita index figures go up substantially while the New England, Middle Atlantic, and West North Central states show sharp declines. (See columns 6 and 7.) In terms of shares, which is the more significant way to view the altered distributional patterns, the New England and the Middle Atlantic regions, those with the greatest per capita declines, have a reduction in their collective share from 32.6 percent under the previous grants to 22.1 percent of the CDBG total. The three southern regions, however, increase their share from 29.2 to 36.8 percent of the total (columns 2 and 4).

The shift in shares is the net result of previous funding levels under the categorical programs and the poverty and overcrowded housing factors, as shown in columns 9 and 10.

Metropolitan Distribution Patterns

During the 1968–1972 base-year period ,the nation's metropolitan areas received 87.4 percent of the funds distributed under the seven urban grants folded into the CDBG program.

Under full formula funding the metropolitan allocation increases from $1.825 billion under the previous programs to $2.24 billion in 1980. However, this results entirely from the increase in the total amount of funding provided. The proportion of CDBG funds going to metropolitan areas declines by 7.4 percentage points because of an 80–20 metropolitan–nonmetropolitan split of the law.

Metropolitan Share. Although metropolitan areas as a whole would receive more money in 1980 under the law as originally passed, the principal beneficiaries within SMSA's are small communities, eligible for discretionary funds. (See Table 8.) The most striking point is that the nearly 600 formula entitlement areas (central and satellite cities and urban counties) collectively would receive less under the formula in 1980 than they had received annually under the previous programs in the hold-harmless base period, declining from $1.659 to $1.612 billion. The share of total funds going to these formula entitlement communities declines from 79.5 to 54.6 percent.

Small Community Shares Combined. As a class, small communities gain sig-

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TABLE 8

Categorical or Folded-in Grant Distributions Compared with Projected 1980 Allocation of CDBG Funds within Metropolitan Areas

Recipient Metropolitan Area, by Type	*Number*	*Folded-in (millions of dollars)*	*Percent of Total*	*Formula (millions of dollars)*	*Percent of Total*
Metropolitan areas					
Central cities	362	1498	71.8	1182	42.2
Satellite cities	152	94	4.5	140	5.0
Urban counties	73	67	3.2	290	10.3
Rest of SMSA's[a]		166	7.9	628	22.5

SOURCE: The folded-in grant allocations and the indices were calculated directly from hold-harmless determinations and data elements used by the Department of Housing and Urban Development in making first-year CDBG allocations. The fiscal 1980 allocations were computed by applying the first-year data on objective needs to a projected fiscal 1980 appropriation.

[a]Places under 50,000 population that receive discretionary grants.

nificantly under the CDBG program as projected. Combining the metropolitan and nonmetropolitan allocations, communities under 50,000 would receive $1.18 billion in fiscal year 1980.[4] This means that 42 percent of total CDBG funds in that year would be allocated to these small communities on a discretionary basis. Under the categorical grants, 22 percent of the funds went to these small communities. If we assume that all of the $290 million projected to be received by urban counties in 1980 will be spent in small communities, the allocation for small communities would be $1.45 billion, *or 52 percent of the total.*

Furthermore, if the $60 million allocated to central cities under 50,000 is included, the total increases to $1.53 billion, or nearly 55 percent of the entire appropriation.

Finally, if one considers a small city to be under 100,000 population, then approximately $1.8 billion or about two-thirds of the total funds are projected to go to small cities in the sixth year of this program.

Central City Allocations. The principal recipients collectively under the earlier grants were the central cities. This was to be expected under programs focused on reducing urban blight and providing social services. This central city advantage under the categorical programs is shown by the fact that central cities received nearly 72 percent of all funds (see Table 8.) Under the formula system, however, central cities as a group lose both in absolute and relative terms.

The 1980 projections indicate central city losses in the aggregate of more than $300 million, with their share of total funds declining from 71.8 to 42.2 percent. The central cities are the only jurisdictional types within metropolitan

[4] This does not include central cities with a population under 50,000.

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areas to lose funds under the formula allocation system compared to the hold-harmless base.

Analysis of central cities shows important regional differences; losses are not experienced in all sections of the country. Generally, central cities in the East North Central, East and West South Central, Mountain, and Pacific regions show a per capita gain under the formula system. However, the central cities of the New England, Middle Atlantic, West North Central, and South Atlantic regions show a per capita decline, with the greatest aggregate losses among the central cities being in the New England and West North Central regions. For New England, the decline is particularly sharp, its per capita index dropping from 204 to 87 (mean per capita equals 100) and going from the highest to the lowest per capita allocation. Central cities in the South Atlantic region also experience a collective loss relative to previous funding, but the central cities of that region, unlike those of New England, remain well above the per capita mean under the formula because of their high incidence of overcrowded housing and poverty.

Among the three regions in the northeast quadrant (New England, Middle Atlantic, East North Central), the area that contains many of the nation's oldest and most distressed central cities, only one region—the Middle Atlantic—has a central city formula allocation above the national mean. This is the result of the major gain to New York City under the formula in relation to its relatively low (in per capita terms) hold-harmless base.

Formula Changes

As awareness of these full and ultimate effects of the CDBG formula increased, so did the strength of our conclusion that it needed to be revised to give greater weight to the needs of older, declining cities. A number of proposed formula modifications emerged as the third and final year of the initial appropriation approached (fiscal 1977), including the idea of a dual-formula as presented in the Brookings analysis.

In considering formula changes, we begin by looking at the original formula factors. We generally accept the need for a population factor, even though it contributes materially to the spreading effect of the CDBG program. If a city with a population of over 50,000 had no overcrowded housing and no poverty (and there are some satellite cities for which this is nearly the case), that city would still receive an entitlement based on population.

The case for the poverty factor, however, is more complicated. The CDBG program is not customarily seen as a means of eliminating poverty, although it is aimed at alleviating the conditions of physical blight that accompany urban poverty. Viewed from the local level, poverty-impacted communities tend to spend a higher proportion of their budget on services for the poor while deriving relatively fewer tax dollars from this group. Hence, the poverty factor can be said to serve as a proxy for both physical and fiscal need.

The overcrowded housing criterion is, in our view, the most questionable of

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the three formula factors. The extent of poverty and overcrowded housing are closely associated (with a correlation coefficient of .6411). Thus, the overcrowded housing factor has the effect of further weighting the poverty factor, further contributing to the regional imbalance of the original formula.

What is missing from the formula is a measure of physical need to serve as an indicator of the physical condition of a community and its facilities—streets, curbs, sewers, schools, housing, etc. The best available statistical indicator for this purpose is census data on the amount of housing stock built prior to 1939. (Because of the low level of housing construction during the 1930s, the pre-1939 housing figure can to a significant extent be viewed as a pre-1930 figure.) Not all housing built before 1939 is deteriorated or deteriorating, but, based on our analysis, the age of housing appears to be linked to the rehabiliation needs of urban communities and to the physical development purposes of the CDBG program.

The Dual-Formula Approach

Among the alternative formulas tested, the one that seemed most feasible as a *basic approach* to formula change is referred to in our analysis as a "dual-formula."[5] It retains the formula in the original law and at the same time adds a new formula of population, poverty, and pre-1939 housing (weighted twice). The second formula, as can be seen, simply replaces the overcrowded housing factor with pre-1939 housing, which, as discussed, is an indicator of the physical dimension of urban need. Each formula entitlement area receives CDBG funds according to whichever formula yields the highest amount in its particular case. This means that the total entitlement allocation under the two formulas increases to approximately $1.88 billion.[6] There is then an important residual effect—the metropolitan discretionary fund is automatically reduced, in this instance to $360 million, or to about 16 percent of the total metropolitan allocation.

In testing this approach, it seems that the three southern regions and the Mountain and Pacific areas would continue to be better off under the existing formula; the Northeast and North Central regions, however, would gain under the dual formula. This pattern is evidenced by the number of entitlement communities falling under each formula. Of 512 central and satellite cities, 241 communities, primarily from the southern and western states, would receive their allocations under the existing formula while 271 would benefit from the alternative age-related formula. Of these 271 communities gaining, 218 are in the Northeast and North Central regions.

Under the dual-formula approach, the nation's central cities increase their

[5] See Chapter 6 of Richard P. Nathan et al., *Block Grants for Community Development* (Washington, D. C., 1977) for the eight other formula alternatives tested.

[6] For purposes of these calculations, we have assumed total funding of $2.95 billion in the year in which full formula funding goes into effect—i.e., the hold-harmless feature in the law, as scheduled, is completely phased out.

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share of the metropolitan funds by nearly 10 percentage points from a 52.8 percent share under the present system to 62.2 percent (see Table 9).

Supplemental Funds for Needy Cities

There is a second set of formula issues yet to be considered. Once a program exists for distributing money for community development on a broad basis, as a practical political matter, there would appear to be a much better opportunity than would otherwise be the case to concentrate some amount of additional funds on the cities with the most acute development needs. One such option studied in our research is a formula add-on limited to the neediest cities. The Carter administration in its budget revisions for fiscal year 1978 proposed an add-on, although not a formula add-on. It proposed that $400 million be added to be allocated by the secretary of HUD on a discretionary basis according to urban needs and the related development potential of the cities applying. There are obviously many positions that can be taken on the issue of whether and to what degree supplemental CDBG funds should be distributed on an automatic or formula basis, including the possibility of a full formula allocation or less precise approach involving a statistical "test of admission," on which basis the secretary of HUD could then make grants to eligible "needy cities" on a discretionary basis. Our approach as tested in the first-year research program, was to concentrate on a full-formula system for distributing supplemental funds, allocating $500 million to demonstrate the way in which formulas could be devised for this purpose. The formula for our supplemental funding approach is presented in detail in the Appendix to this article.

Using supplemental funding on a needs-tested basis would also provide an opportunity to take into account other important characteristics of urban distress. Population change is stressed in this process; it is used twice in the allocation of supplemental CDBG funds. Among CDBG entitlement cities there are important

TABLE 9

Comparison of Allocations to Metropolitan Areas Under the Existing CDBG Formula with the Dual-Formula Alternative (Projected Fiscal Year 1980)

Community Type	*CDBG Percent Total*	*Dual-Formula Percent of Total*
Central cities	52.8	62.2
Satellite cities	6.3	7.5
Urban counties	12.9	14.3
SMSA discretionary	28.0	16.0

SOURCE: Calculated from data used by the U. S. Department of Housing and Urban Development in making first-year CDBG allocations. Data on pre-1939 housing are from *1970 Census of Housing, Detailed Housing Characteristics*, Series HC (1) B, Tables 35, 43, and 62. Township housing data are from *1970 Census of Population and Housing, Census Tracts,* Series PHC (1), Table H-2.

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socioeconomic distinctions between the 150 cities losing population and the 356 cities that gained population between 1960–1970 as shown in Table 10.

Overall, the income of blacks is about 60 percent that of whites; CDBG entitlement cities with declining populations tend to have much higher proportions of blacks and of lower-income persons. The declining cities also have lower median housing values than the growing cities.

While these data are significant, they reflect the situation at a given point in time. Examining the rate of change of two important resource factors, income and housing values, it can be seen that between 1960 and 1970 per capita income increased almost 5 percent faster in the growing cities (column 4, Table 10) than in the declining cities; home values increased nearly 6 percent faster (column 6) in the growing CDBG cities.

In sum, distressed cities are characterized by old age, an increasing concentration of the socially and economically disadvantaged, and population decline (or slow growth). The dual formula described earlier covers two of these aspects of distress: poverty and community age. The system for distributing CDBG supplemental funds uses all three factors.

We would stress that in respect to both the dual formula and the supplement, our discussion presents what to us are reasonably feasible and straightforward *examples* of types of formula changes. We regard the allocation system as a framework of movable parts, the positions of which depend upon the nature and degree of the distributional shift desired. The formula for a $500 million supplement is one among an unlimited number of ways in which a further skewing to deal with "urban need" could be achieved.

These formula ideas are intended as illustrative of the ways in which the present law could be changed. Our basic approach has been that the law provides a framework of movable parts which can be modified in ways that achieve different people's objectives as to how urban needs should be met. To us, the most important points are the dual formula and the provision of supplemental CDBG funds for "needy" cities. Both the Ford and Carter administrations in 1977 rec-

TABLE 10

Important Characteristics and Rates of Change during 1960–1970 for CDBG Cities, Grouped by Population Gains or Losses

Group	Percent Population Change (1)	Average Percent Black 1970 (2)	Per Capita Income 1970 (dollars) (3)	Percent Income Change 1960–1970 (4)	Median House Value 1970 (thousands of dollars) (5)	Percent Value Change 1960–1970 (6)
150 losers	−6.7	17.3	3062	57.0	15.9	32.5
356 gainers	20.5	10.2	3354	61.7	18.8	38.2

SOURCE: Population, income, and housing data from U. S. Bureau of the Census, *County and City Data Books, 1962 and 1972*, Table 6.

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ommended a version of the dual formula, in both cases with a stronger urban focus than that of the dual formula originally developed as part of the Brookings monitoring study of the block grant program. The principal reasons for the stronger urban focus of these proposals is that instead of using population as one of the three factors (along with pre-1939 housing and poverty), they use population *change*, thus giving recognition to population decline or slow growth in the basic formula.[7] HUD Secretary Patricia Roberts Harris also presented a proposal to the Congress for supplemental CDBG funds. Unlike the suggestion for a formula-based supplement presented in this article, the Harris proposal is in the form of discretionary funds for so-called "Action grants" for special projects*.

Appendix

The following two-step procedure was devised to determine both eligibility for supplemental funding and the amount the eligible jurisdictions would receive.

Step 1. Constructing an Eligibility Index

The first step is the determination of eligibility. We confine eligibility to metropolitan cities. The following equation is used to scale the needs of metropolitan cities.

$$\text{Eligibility index} = \frac{(\text{percent of pre-1939 housing}) \times (\text{percent of poverty})}{100 + \text{rate of population change}}$$

Such an eligibility index number was determined for all formula entitlement metropolitan cities.

Then, assuming that the eligibility threshold is set at the mean, 196 cities qualify, of which 122 (62 percent) are below 100,000 population, 177 are central cities, and the rest (19) are satellite cities. Regionally, the Northeast and North Central states have 121 cities that qualify—61.8 percent of the total.

Step 2. Calculating Individual Allocations

The second step is the calculation of individual allocations for the eligible cities. We use a formula equally weighting population, poverty, and pre-1939

[7] The Ford proposal has the strongest urban focus of the three, as it uses population *decline;* the Carter version uses population *decline or below average growth;* the original Brookings version uses *straight* population.

* The material presented reflects the findings and interpretations of the authors, not the views of the trustees, officers, or other staff members of the Brookings Institution. The research is being performed under a contract with the U. S. Department of Housing and Urban Development. The first report on this study, on which this article is based, is Nathan et al., *Block Grants for Community Development.*

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housing and derive a coefficient for each eligible city to be applied against the total fund of $500 million. However, before applying these coefficients, they were adjusted for two other purposes.

Population Decline Adjustment. For the 196 eligible cities, the average population change was -2.1 percent between 1960 and 1970. A rate of population change ratio is calculated:

$$\text{Population change ratio} = \frac{100 + \text{rate of change for individual city}}{100 + \text{mean rate of change for participants (i.e., 97.9)}}$$

Then the allocation share is determined as follows:

$$\text{allocation share} = \frac{\text{coefficient number}}{\text{population change ratio}}$$

Scaling Adjustment. To increase this spread (make it more redistributive) and reduce the "notch" problem (i.e., for the 197th city) a further formula adjustment is used to scale the final allocations. This is done by multiplying the allocation share by an "eligibility adjustment factor," which is the ratio of an individual city's index to the mean index of the eligible cities.

$$\text{Scaling adjustment factor} = \frac{\text{individual city eligibility index}}{\text{mean eligibility index of eligible cities}}$$

Thus the final allocation is determined:

$$\text{Individual supplement} = \left(\frac{\text{allocation coefficient}}{\text{population change ratio}}\right) \times \text{scaling adjustment factor} \times \$500 \text{ million}$$

Applying this approach to the 196 cities, East St. Louis (the top city) receives $42.48 per capita; Seattle (the bottom city), $5.64 per capita.

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Tactical Nuclear Weapons and Deterrence in Europe

PHILIP W. DYER

The deterrence of Soviet aggression in Europe has been a cornerstone of American policy for thirty years. It was undoubtedly World War II and specifically the reality of Soviet troops as far west as the Elbe that created the peculiar "mind set" in American foreign-policy makers that envisions the Russians straining desperately at the leash waiting for the opportune time to "continue" the conquest of Europe. In this view, only the stalwart forward defenses of NATO coupled with the ever-alert SAC have prevented the powerful bear from crashing through to the English Channel and possibly beyond. Given such a heavy burden for NATO it is no wonder American policy makers become glassy eyed as the alliance proves unable, throughout twenty-eight years, to bring its forces and its doctrines into some kind of congruence that would make sense to the world, or at least to ourselves. In vain have we struggled through all those years to provide the right mix of technology, manpower, and doctrine to make NATO rational.[1] This article argues that of all the frantic and

[1] There is sometiing to be said for the position that much of what happens in politics is not rational. Some would argue that the force levels and weapons employed in Europe are the result of many pressures and various forms of internal political bargaining rather than of some rational strategy. While such a position has merit it is also true that when a NATO commander appears before congressional committees and justifies additional expenditures for improving weapons systems he does so by marshaling arguments perceived to be rational by congressmen. If we can demonstrate that those arguments are not so rational after all, it becomes more difficult, regardless of other pressures, for Congress to act on the basis of them. Even if "rational-

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irrational things done in the name of deterring the Soviets in Europe, the most dangerous and wasteful has been the emplacement of tactical nuclear weapons on European soil.

The Political Setting for Deterrence

In an effort to see these weapons and their role in NATO in a different light we shall first consider the problem of deterrence in Europe generally, and then focus on the alleged roles of tactical nuclear weapons in that deterrence. It is very difficult to see under what combat conditions, especially in Europe, these weapons would ever be employed.[2] Here the task is to determine if perhaps some deterrent role can be found for the weapons despite the difficulty of envisioning a combat role.

Perhaps the best way to begin answering the question of what stops the Soviet Union from invading Western Europe tomorrow is to ask what stops NATO from invading Eastern Europe tomorrow? It is not enough simply to say that we are peace loving or we do not have any desire to invade, because those are simply expressions of intentions that are difficult to validate. The Russians might well say the same thing about their own intentions, but we naturally do not believe them. As military analysts frequently point out, not only are intentions difficult to assess but they also can change rapidly and therefore should not form the basis of one's policy.[3] Our intentions aside, it might not be too simplistic to argue that essentially what deters us, or any nation, from launching a war is the result of some form of cost-benefit calculation however rudimentary, crude, and imprecise that calculation might be.

Eastern Europe—unlike the Middle East, for example—has few natural resources that the West needs. Therefore the primary benefits would have to be the people, their strategic position, and their industrial base, especially in East Germany. But how, precisely, would the people and the industrial base help us? Even assuming we could occupy the area with a minimum of troops, how could we profit from such an occupation? Our economy does not particularly need what Eastern Europe produces and it is difficult to see what else we would stand to gain except a little better strategic position vis-à-vis the Soviet Union. Thus just

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ity" is only a façade behind which other motivations hide it is useful to destroy the façade wherever possible and perhaps thus get closer to whatever is hiding behind it.

[2] Philip W. Dyer, "Will Tactical Nuclear Weapons Ever Be Used?" *Political Science Quarterly*, 88, no. 2 (June 1973), 214–229.

[3] Here is one presentation out of the many that could be selected supporting this common position: "First, let me say a few words about the threat in Europe, that is, the capabilities of the potential enemy. . . . I am not speaking of enemy intentions, for planning can be based on intentions only when one knows what they are. . . . Intentions can and do change overnight, whereas it takes years to develop and refine military capabilities." General Lyman Lemnitzer, "From the American Point of View," in Lord St. Oswald (ed.), *The Soviet Threat to Europe* (London, 1969), pp. 21–33. Also see the discussion in Thomas C. Schelling, *Arms and Influence* (New Haven, Conn., 1966), p. 35.

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considering the problem from the benefit side we can see that there is not much to be gained even if we could seize that area tomorrow without a fight.

However, when costs are calculated the idea becomes clearly absurd. The immediate and direct cost would be, undoubtedly, a large-scale conventional war with the Warsaw Pact army that would represent a considerable expenditure of lives and money. That cost in itself when weighed against the marginal value of the prize to us is probably enough to deter NATO from undertaking such an adventure. However, an even far greater cost lurks down that road since the world's two nuclear superpowers would be involved in an armed conflict over an area of the world that at least one of them apparently considers vital to its own security. Obviously, then, into the calculation must go at least the possibility that such a war might end in a thermonuclear exchange between the United States and the Soviet Union. Thus, we have a marginal benefit overbalanced by a substantial real cost coupled with the possibility of a catastrophic potential cost all of which results in the decision that it would be foolish from our perspective to engage in such an undertaking.

From the Soviet perspective, an invasion of Western Europe by the Red army would entail a similar cost-benefit analysis. Like Eastern Europe, Western Europe is not richly endowed with natural resources, so the gain there, too, would be primarily the people, the industrial base, a superior level of technology, and possibly an improved strategic setting. How, precisely, would the Soviet Union be strengthened by occupying such a large territory as Western Europe? Since it is naive to imagine that the local population would be friendly, it would be necessary for the Soviets to field a rather substantial army of occupation. Puppet governments might well be used, of course, but the experiences of Hungary and Czechoslovakia have shown the Soviets that puppet governments are not always reliable and certainly do not eliminate the need for large military forces to be stationed in a country.

Although the USSR is a superpower it is not Superman. The only way in which Russia is a superpower compared to Western Europe is in its possession of an awesome strategic thermonuclear strike capability. Otherwise, it is a country of approximately 250 million people with a GNP of approximately $650 billion. Western Europe, even without Great Britain, is composed of more than 270 million people with a combined GNP in excess of $800 billion. The Russians occupying such a region conjures up a vision of one whale trying to swallow another. In the occupation of Western Europe nuclear weapons are not especially relevant, so that two roughly equal entities are to be considered. Here the experience of France in the nineteenth century and Germany in the twentieth, both of whom enjoy a more favorable geographic location, might be useful in showing the difficult and short-lived nature of any modern attempt to occupy all of Europe.

Even if the Russians were willing to field and maintain indefinitely a huge army of occupation, there are several other considerations that they would have to weigh as they attempted to figure out the benefit to them of seizing Western Europe. One problem whose magnitude has become obvious in recent years is the

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question of energy. The Soviet Union is roughly self-sufficient in oil, at least for the present, but it certainly does not have the resources to slake Europe's enormous thirst for that mineral. Would Middle Eastern oil flow to a Soviet-occupied West Europe in its present quantities? That would dépend, among other things, on what the United States chose to do. Assuming that a Soviet rush to the English Channel would have overrun NATO and 250,000 American troops (the costs will be considered later) America might well use its naval forces to reduce the flow of oil. An occupied Western Europe without sufficient oil could turn out to be more of a liability than a benefit to the USSR.

Another equally serious matter is for the Soviets to determine specifically how they would benefit from such an occupation even if the problems of the army and energy were solved. Are Western products to be confiscated and sent directly to the Soviet Union so tht each Soviet citizen will drive a Volkswagen, Fiat, or Renault? Are the economies of that whole area to be combined into one super common market? Are machine tools and factories to be disassembled and shipped to the Soviet Union as they were after World War II? (The army of occupation would have to be increased.) Does the Soviet Union really need such "benefits"? How does the substantial economic strength of East Germany presently help the Soviets? If the Russians need Western products so badly why have they not confiscated East German goods and shipped them eastward? Would the Russians be strengthened internationally? Would all future military calculations have to assume that the armies of Western Europe must be added to Soviet forces when put in the balance? But if Europe had been overrun what future war would involve armies in that area? Would it not be a question of the two thermonuclear powers eyeing each other across the Atlantic? In such a confrontation how does the occupation of Western Europe strengthen the Russians? Would the Soviet nuclear threat be so appreciably enhanced by the seizure of the French *force de frappe* (if that had not already been employed) that Russia's position vis-à-vis the United States would become noticeably improved? These questions are not easy to answer but hopefully they at least suggest that from the Soviet perspective the seizure and occupation of Western Europe might not produce benefits of such a clear and enticing nature as to make that adventure worthwhile even if the costs were negligible. However, the costs are not insignificant and it is to them that we shall now turn.

What are the tangible direct and potential future costs of a Soviet drive to the Channel? Unquestionably such an undertaking would at least cause a conventional war of substantial proportions. On the first day of hostilities the Warsaw Pact would have 576,000 troops (339,000 Soviet) deployed against 660,000 NATO troops (200,000 American). Within thirty days the figures might be 1,067,000 Warsaw Pact (677,000 Russians) against 1,045,000 NATO (285,000 American); while sixty days after the invasion the figures would not have changed significantly—1,241,000 Warsaw Pact (842,000 Russians) against

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1,105,000 NATO (345,000 Americans).[4] It should be noted that because of their wide geographic distribution not all of the NATO troops would necessarily be engaged against the enemy. In any event if one wanted to question the reliability of Eastern European forces in a clear war of aggression against the West the Soviet Union would, indeed, be outnumbered by NATO forces fighting for the defense of their own homeland. The first real cost the Soviets would face, then, would be a substantial conventional war that they could not be guaranteed of winning. A second possible cost, as with the earlier example, derives from the fact that the world's two nuclear superpowers would be shooting at each other in an area of the world that one of them, at least, has indicated is of crucial importance to its own security. Thus, just as with a NATO invasion of Eastern Europe here, too, there would be a reasonable chance that the hostilities might culminate in a thermonuclear exchange. The Soviets, therefore, would be faced with the real direct cost of a large conventional war in Europe and with the possibility of a catastrophic thermonuclear war. These two costs, one actual and one potential, weighed against the doubtful benefits described earlier help to show why there has been no war in Europe to date.[5] But if deterrence in Europe is a function of doubtful benefits, real conventional costs, and potential thermonuclear strategic costs, then how do the tactical nuclear weapons presently stationed in Europe fit into this deterrence picture? In a word, they do not; and it is to a consideration of this peculiar weapons system and its alleged roles in deterrence that we shall now turn.

The Weapons and Their Proposed Roles

Before discussing the proposed roles of the tactical nuclear weapons stationed in Europe it is necessary first to bring the reader up to date on the weapons themselves. The ground-launched weapons consist primarily of artillery and surface-to-surface rockets and missiles. The artillery includes the 155 millimeter and 8 inch howitzers of both the towed and self-propelled variety. These are standard artillery weapons organic to any American division-size unit, as well as to artillery units at higher levels. The nuclear shells for these weapons have been engineered to fit the conventional artillery piece without modification, thus making all 600 or so of these weapons presently in Europe nuclear-capable. The

[4] These figures came from Jeffrey Record, *U. S. Nuclear Weapons in Europe: Sources and Alternatives* (Washington, D. C., 1974), p. 17.

[5] To those who would object that such a simplistic cost-benefit calculus does not allow sufficient room for ideological considerations that may be crucial in understanding Soviet behavior, I would respond that neither Soviet actions since the end of World War II nor their rhetoric, especially in the twentieth and twenty-second Party Congresses, convinces me that they have such "a passion for souls" that ideological convictions about world dominant messianic communism play any significant role in their calculations.

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range of the weapons is from point blank (not advised with a nuclear round) to approximately 17,000 meters and the yield varies from less than one kiloton (kt) to more than 10 kt.

The second category of ground-launched weapons are the rockets and missiles including the Honest John and the Sergeant; their replacement, the Lance; and the longer-range Pershing; as well as the Nike-Hercules which is primarily an air defense weapon.[6] The Lance and the Pershing have ranges from 5000 meters to almost 80 miles and 110 miles to 450 miles, respectively, with yields of 1–100 kt and 60–400 kt. In addition there are atomic demolitions of various sizes about which not much is known.[7]

The tactical air capability greatly increases both the range and yield of atomic weapons available to the Supreme Allied Commander Europe (SACEUR). The two principal weapons are the Air Force's F-4 and F-111 which have respective strike radii of 800 and 1700 miles and are each able to carry bombs that vary from .1 kt to 1 *megaton*.[8] Thus, at least in theory, the Soviet ports of Murmansk and Odessa could disappear under a 1 million ton TNT-equivalent mushroom cloud and yet from our perspective the war would still be a tactical nuclear one. It is precisely because of the ranges and yields of these weapons that there is no probable war in Europe when the weapons would ever be used.[9]

What deterrent roles do observers see for such an impressive array of weaponry? Probably the least frequently used rationale is that the weapons deter the Russians from using their own tactical nuclear weapons. One reason this argument is not made often is that the Russians are believed to have a conventional military advantage in Europe that obviates their need to use the weapons. Also, if they had decided that Western Europe was worth taking (despite the great costs discussed above) there would be little sense in their destroying what they had set out to conquer. Further, the Russians developed their weapons after ours and even now the yields of their weapons (most exceed 20 kt) as well as their doctrine, which emphasizes mass employment of nuclear weapons, seem to indicate that the planned Russian use of the weapons is not conducive to the seizure of an intact Western Europe.[10] Thus it would appear that the Soviets would probably not use these weapons first if they had set out to overrun Western

[6] Even our allies have agreed to buy the Lance according to *The New York Times*, June 7, 1973, p. 7.

[7] Performance characteristics of these weapons differ somewhat from source to source. To avoid possible criticism the author has used only data from the public domain even if it is somewhat inconsistent. For example, see Record, *U. S. Nuclear Weapons*, pp. 20–25; *The Defense Monitor*, 4, no. 2 (February 1975), 5; and U. S. Congress, Joint Committee on Atomic Energy, *Hearings Before the Subcommittee on Military Applications*, 93d Congress, 1st Sess., May 22 and June 29, 1973, part 1, p. 9. (Hereafter cited as *Hearings*.)

[8] For comparison it is well to keep in mind that the Hiroshima and Nagasaki bombs yielded approximately 14 kt.

[9] See note 2, above.

[10] For a recent discussion of Soviet doctrine see A. A. Sidorenko, *The Offensive (A Soviet View)* (Washington, D. C., 1973).

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Europe. Therefore few Western strategies seriously suggest that the primary role for our tactical nuclear weapons is to deter the Soviets from using theirs.

The deterrent role most frequently cited for these weapons is to inhibit the Russians from launching that large-scale conventional invasion that NATO might otherwise be unable to contain. The precise role to be played by our tactical nuclear weapons takes one of three different forms in this view and it is well to consider each form separately and carefully because they are often presented with such glib ease that they sometimes escape examination. The first possibility, which is still expressed from time to time, apparently envisions a unilateral use of these weapons by a NATO that is then able to stop the onslaught.[11] The questions that arise are not only how realistic is the assumption that the Russians will not use their nuclear weapons, but also how willingly will the West Europeans agree to our use of these weapons knowing that Soviet doctrine and weapons could make quite a mess out of Europe? Oddly, analysts often argue as if the difficult decision is the Russian one of whether they ought to retaliate, with nuclear weapons, whereas it seems that the really awesome decision would be the one to *initiate* this form of war.[12]

The second variation of this argument recognizes that the Soviets may well use their weapons and then admits that it is not known at this time whether the use of these weapons helps the attacker or defender. However, this argument then suggests that when the Russians recognize that we are serious about defending Western Europe, they will discover that the war will cost them more than they had originally calculated and will probably call it off without having accomplished their goals.[13] The hope is that the actual and potential escalation in the level of violence will deter the Russians. What is frequently overlooked, of course, is that much of the additional violence will be directed against the

[11] A recent restatement of this curious position that may have made sense twenty years ago can be found in Wynfred Joshua, *Nuclear Weapons and the Atlantic Alliance* (New York, 1973), pp. 53–54. Dennis Gorley believes that even Richard Nixon envisioned NATO's first use of these weapons but does not indicate whether the former president also saw their use as unilateral. See Dennis Gorley, "NATO's Tactical Nuclear Option: Past, Present and Future," *Military Review*, 53, no. 9 (September 1973), 3. The following statement of this position is revealing: "In the event of a conventional attack which cannot be held by conventional means, tactical weapons would be used by the West in very limited numbers. . . . *What happens next is not quite so clear and is believed to be still under study* in the NPG [Nuclear Planning Group of NATO]." (Italics added.) Bernard Burrows, *The Security of Western Europe* (London, 1972), p. 64.

[12] Consider, for instance, this very curious wording: "In response [to NATO's use of nuclear weapons] the Soviet Union could, of course, *consider escalation*." (Italics added.) Is it the Soviet Union that would be escalating the war or the West which had first introduced the weapons? See Wynfred Joshua, "A Strategy for the West: An American View," in Richard B. Foster, Andrew Beaufre, and Wynfred Joshua (eds.), *Strategy for the West: American Allied Relations in Transition* (New York, 1973), p. 102.

[13] Two, among many, examples of this argument are: Albert Legault and George Lindsey, *The Dynamics of the Nuclear Balance* (Ithaca, N. Y., 1974), p. 142; and General Andrew J. Goodpaster in *Hearings*, part 2, pp. 55–56.

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peoples of Europe and if the war continues for any length of time Europe may find itself in the same position as the unfortunate Vietnamese town of several years ago which was destroyed in order to be saved. As we shall see in the discussion below there are far less traumatic ways to indicate to the Russians that the level of violence may spiral upward.

The final variation in this series of supposed deterrent roles sees our deployment of these weapons as an indication to the Soviets that we are willing to cross the nuclear threshold in defending Europe—thus raising the possibility of Armegeddon. It is odd how often such a view is presented today,[14] odd because one of the strong arguments originally presented in defense of these weapons was that they would offer an alternative to massive retaliation by giving us other viable options short of holocaust for the defense of Europe. Now, later analysts seem to be suggesting that the weapons are primarily an indication of our willingness to resort to strategic thermonuclear bombing of the USSR in order to protect Europe. Several problems emerge from this apparent reversal of our earlier position. If the purpose of battlefield atomic weapons is essentially to demonstrate our willingness to go the final mile do we need 7000 weapons to do that? The second, and possibly even more serious, problem is precisely how does the firing of a 155 mm atomic shell signal to the Russians that the next step may be up to SAC? Wouldn't it be more reasonable to assume that since we have so many of the weapons, and since we have developed the concept of limited nuclear war to such a fine point, the use of tactical nuclear weapons signals nothing other than that we are using tactical nuclear weapons? Finally, if our intent is, indeed, to show our willingness to go all the way, why be unnecessarily vague about it? The best way, it would seem, to truly show our intention to employ strategic bombardment is to fire a strategic missile. Surely rung number 26 on Herman Kahn's ladder makes more sense, if that is our true intention.[15] It may not necessarily be true that we want to defend Europe with a thermonuclear exchange but if we do, there are far more effective ways to demonstrate this fact to the Russians than by employing tactical nuclear weapons.

Despite the confusion over their deterrent roles, however, according to another theory the weapons themselves are real and tangible and their existence in Europe represents an American commitment that the Europeans themselves do not want to see changed.[16] For want of a better term this perspective should be dubbed the "inflation argument." The crucial question that begs to be answered

[14] In addition to the Joshua and Burrows pieces cited in note 11, a few other examples would be Robert Ball, "Rethinking the Defense of Europe," *Fortune*, February 1973, p. 63; and Record, *U. S. Nuclear Weapons*, pp. 58–59. Also see *The New York Times*, December 2, 1973, p. 1.

[15] Rung 26 is a "demonstration attack on zone of interior" with a strategic nuclear weapon. Kahn sees at least forty-four different rungs on an imaginary ladder of increasing violence between "ostensible crisis" and "spasm war" or wargasm. See "Escalation as a Strategy" in Henry A. Kissinger (ed.), *Problems of National Strategy* (New York, 1965), pp. 17–33.

[16] Burrows, for instance, believes that these weapons "restore the credibility of the deterrent." *The Security of Western Europe*, p. 67.

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is this: To what, exactly, do these weapons commit America? Possible answers to that question have already been considered and the results were not very satisfying. Usually, though, that question is avoided and the argument remains that we must show the Soviets we are serious about defending Europe and surely one of the most dramatic ways to do this is to put a pile of nuclear weapons over there.

The inflationary nature of this argument is clear to all those who remember that many years ago that same reasoning was used to justify placing American troops in Europe. We had to show the Russians that we were serious about defending our allies and what better way could we do this than by stationing 300,000–400,000 troops on the forward edge? It is probably inevitable in our era that many things lose their value over time. Now apparently 250,000 men are not enough to convince the Russians of our seriousness but they must be augmented by 7000 tactical nuclear weapons. One wonders if in another ten or fifteen years it may not be necessary to position a substantial strategic capability in Western Europe to prove our intentions.

It may well be that many things lose their value through time but it is equally true that some things remain constant and some even increase in value. That America will fight for Western Europe is as well demonstrated today by 250,000 men as it was twenty-five years ago. And it is certainly much more convincingly demonstrated by the soldiers than it is by the atomic weapons for two reasons. One is that the soldiers can neither be destroyed in place nor evacuated—we simply do not have enough airlift capability to remove that many Americans immediately after hostilities begin. The second follows obviously; there is no alternative for the soldiers but to fight or surrender, while battlefield "nukes" do not have to be used at all. The existence of a sizable American troop commitment in NATO, together with the other contributions, of course, guarantees (unlike tactical nuclear weapons which can guarantee nothing) that a Soviet invasion will create a substantial conventional war and raise the possibility of a total war which, as has been suggested before, is more than enough cost to deter the Russians from exploiting the doubtful benefits involved in invading and occupying Europe. Therefore, it would appear that despite the passage of time America's commitment to Europe is still more clearly demonstrated by troops than by an ambiguous weapons system.[17] If, however, "commitment" in this argument means really willingness to defend using all weapons up through nuclear ones then we must refer to what has already been said about more clearly indicating that intent.

As for the last part of this argument, that the Europeans themselves want these weapons to remain and that removing them would be disastrous for European stability, it may well be that this is no issue at all. What the Europeans undoubtedly want is to get under the American nuclear umbrella. Tactical nu-

[17] This article will make no attempt to consider the very important question of how many troops are necessary to keep deterrence credible.

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clear weapons are the best they have been able to do because that is all that we have clearly offered. Those Europeans who are pleased to see the weapons in Europe, and their numbers may not be large, derive comfort from the fact that the weapons are at least a link to the strategic ones. There are probably very few Europeans who are enthusiastic about tactical nuclear warfare per se no matter how often Americans point out all the good features of that form of insanity.[18] The French have certainly displayed something less than enthusiasm for the idea. At least since 1956 and operation "Carte Blanche," many European observers have clearly realized that for them there may be little or no distinction between tactical and strategic nuclear warfare.[19] Therefore, there may be no substantial European devotion to these particular weapons but there is an eagerness to use America's strategic nuclear umbrella as a deterrent. We do ourselves and our allies a disservice by either being committed to thermonuclear exchange in defense of Europe when we say we are not, or by letting our allies think they may be under the umbrella when actually they may still be in the rain.

The final argument for the deterrent value of these weapons is one that has become more popular in recent years, perhaps as a result of all of the problems that do not disappear. It is the "confusion argument" which suggests that even if all of the objections raised so far are true the weapons still have substantial deterrent impact precisely *because* of the ambiguity surrounding them. This argument is ingenious for making a virtue out of a vice. Especially popular with General Andrew Goodpaster, former SACEUR, but not limited to him, this position essentially argues that since we do not know what we are going to do, the Russians certainly do not and out of this resultant ambiguity a stable deterrent emerges.[20] Another general has worded it this way: "I happen to believe, personally, that an uncertainty in the enemy planner's mind that we might respond with our nuclear artillery or other nuclear weapons creates an uncertainty as to how the war will go, which, I think makes him more deterred, because he cannot control the course of events."[21]

It is probably true that we do not know what we would do if Western Europe were invaded tomorrow. Despite all the position papers and all the labors of NATO's Nuclear Planning Group, the NATO commander of today probably

[18] A forceful presentation by a European who expresses grave reservations about these weapons can be found in John L. Holst, "Malaise in Europe: Diagnosis and Prognosis" in Foster, Beaufre, and Joshua, *Strategy for the West*, p. 172. Holst does, however, believe he sees some hope in a new generation of "mini-nukes." Evidence of other European reservations can be seen in *The New York Times*, December 2, 1974, p. 34.

[19] In addition to Holst, see Gordon C. Craig, "NATO and the New German Army," in William W. Kaufmann (ed.), *Military Policy and National Security* (Princeton, N. J., 1956), p. 226.

[20] For two of his presentations of this idea see Andrew J. Goodpaster in *Hearings*, part 2, p. 56, and "The Defense of Europe," *The Royal United Service Institution Journal*, March 1971, p. 36.

[21] Statement by Major General Edward B. Giller, USAF, in *Hearings*, part 1, p. 11.

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still suffers from the problem identified by Roger Hilsman seventeen years ago—he is not sure what kind of war he will be called upon to fight.[22] The first use of nuclear weapons, not to mention an entire nuclear war, over the continent of Europe is an event of such awesome and catastrophic magnitude that it is difficult to see Europe's friend, the United States, beginning such a thing. So we can agree with the military planners that we are not sure what we would do.

The second part follows logically. If we are not certain what we are going to do the Russians cannot be certain what we are going to do either. They know relatively confidently that there will be a conventional war of some magnitude and beyond that they, too, can only guess. Now we also know for a fact that there has been no war in Europe for the last thirty years, or if one wants to word it more positively that the Russians have been "deterred" from attacking Europe for the past thirty years.

The flaw in this argument emerges when we causally link our confusion over tactical nuclear weapons specifically to the deterrence of the Russians. There is no way, of course, to prove or disprove empirically that our present confusion deters the Soviets but there are some logical grounds to question that position. Throughout those thirty years of deterrence America's posture has changed from having only a small monopolistic nuclear capability and almost no conventional capacity (1945–1949); to a growing strategic nuclear and conventional capability (1949–1954); to a large strategic nuclear capacity, a substantial conventional capability, and an increasing tactical nuclear capability, but a strategic doctrine that called for massive retaliation (1954–1960); to an overwhelming strategic and tactical nuclear capacity, increasingly matched by the Soviets, a still considerable conventional capacity and a strategic doctrine of flexible response or graduated deterrence (1960-present).[23] Despite (or as some might argue because of) the variety of weapons, forces, and doctrines, throughout that period there was peace in Europe. If we are going to make the claim that the present confused doctrine and weapons system is really deterring the Russians then we have to say that all of the previous doctrines and weapons systems deterred the Russians with equal success. Yet for eight of those years (1945–1953) the Russian army was probably strongest vis-à-vis the West and there were no tactical nuclear weapons in Europe. Thus, one wonders what exactly has changed between 1945–1953 and the present day that requires so much conventional and tactical and strategic nuclear capacity to stop the Russians. The obvious response—that the Russians have so much more tactical and strategic capacity (but not, oddly, more conventional power)—would begin to make Yossarian nervous. It would also be an admission that strength levels in Europe

[22] Hilsman compares it to the German General Staff's nightmare of having to fight a two-front war. The difference is that today's NATO commander may be armed to fight one kind of war and then find he has to fight a different kind. See "NATO: The Developing Strategic Context" in Klaus Knorr (ed.), *NATO and American Security*, (Princeton, N.J., 1959), p. 27.

[23] One might argue with the precise dates of these eras as well as each characterization but those details do not affect the argument.

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are simply reactive to those of the other side. If this is true, removing the tactical nuclear weapons might begin to wind levels down for a change. Finally, if confusion does deter, is it confusion over tactical nuclear weapons that deters or confusion over the fact that the war might become total that stops the Russians? The discussion in the first part of this article seems relevant here.

Another question that must be asked is this: If mixed "baskets" of weapons, strategies, and forces, however inconsistent, have been successful in deterring the Soviets must we assume that *each* ingredient in those mixed baskets is a *necessary* one? Must we assume that every new weapon added to the arsenal plays a material role in deterrence? Might not some of them be redundant? And is each particular basket the only one that is sufficient to deter the Soviets? Even a former secretary of defense has indicated that there is no magic surrounding the figure of 7000 for the number of tactical nuclear weapons stationed in Europe.[24] Of course, this writer would go one step further and suggest that there is no magic surrounding the weapons system at all and that it is an *unnecessary* ingredient in the deterrent basket for Europe.

In addition to the faulty causal link between all the components of our ambiguous basket and deterrence there is also some difficulty with the notion that ambiguity is a significant deterrent.[25] The assumption is that the Soviets must develop more contingency plans to account for various NATO options and the more the options, even if ambiguous, the more the Russians refrain from activity.[26] Usually, it is true of course, that an improved or additional capability developed by one's antagonist must be considered but that is not particularly significant unless the capability entails a critical difference. For instance, if the increased complication in Russian plans caused by tactical nuclear weapons is what *actually* deters them then it is well worth our while to introduce ambiguity. But the argument above concerning the period when there were no such weapons in Europe seems to weaken this perspective. If, however, the increase in confusion is a marginal thing that adds only a little greater difficulty to something they have already decided not to do then its value disappears. The doubling of NATO's ground forces would also complicate plans to overrun Europe. But if the Russians are already deterred from that course by the present size of NATO it does not make sense either militarily or financially to double NATO's ground forces. So, too, if they are already deterred by the prospects of

[24] *The New York Times*, April 25, 1974, p. 7.

[25] See the discussion, for example, in Thomas C. Schelling, *The Strategy of Conflict* (New York, 1963), chap. 8.

[26] Statement by Dr. Carl Walske, chairman, Military Liaison Committee to AEC, in *Hearings*, part 2, p. 49. Although the dean of American strategists, Bernard Brodie, has argued that we should not limit our options through psychological paralysis concerning tactical nuclear weapons, much of his discussion centers around the demonstrative value of the weapons. See Bernard Brodie, *Escalation and the Nuclear Option* (Princeton, N. J., 1966), pp. 123–131. However, this article has already indicated that there are serious problems with this role for tactical nuclear weapons.

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a large conventional war and a possible thermonuclear one, then adding additional confusion into their calculations is redundant and a waste of resources. Finally, because of the extreme ambiguity concerning the weapons as well as the fact that it is we who would probably have to decide on first use one wonders if they do not really complicate our planning a good deal more than that of the Soviets.

Conclusion

Tactical nuclear weapons do not play a sufficiently significant role in the deterrence of Soviet aggression against Europe to justify their expense. With each individual atomic artillery shell costing perhaps $400,000 this weapons system is not cheap.[27] For the kind of resources absorbed by these weapons we must have a greater payoff than the fact that they may have made slightly more complicated Soviet plans for overrunning Europe. The Russians are deterred from invading Europe by a cost-benefit calculation that shows highly ambiguous and doubtful benefits more than offset by the certain cost of a large conventional war and the possible cost of a thermonuclear exchange. Tactical nuclear weapons appear only marginally in that calculation. Additionally their presence in Europe is to our disadvantage insofar as they force the Soviets to consider preemptive tactical nuclear war as a possibly necessary option. Those weapons on Quick Reaction Alert add a destabilizing element to the balance of terror that is not in our best interest.[28] Finally, the security and control of these weapons consumes the equivalent of over one full division of American troops who could be used more profitably in another role.[29] For these reasons, the weapons should be removed from Europe.

[27] That figure was used by Senator Stuart Symington in *Hearings*, part 2, p. 101, where the issue under consideration was a proposed $1 billion program to modernize small nuclear weapons.

[28] Former Secretary of Defense James Schlessinger indicated his willingness to remove 250 Pershing missiles from this ready status. *The New York Times*, April 25, 1974, p. 7.

[29] Record, *U. S. Nuclear Weapons*, p. 69.

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Decision Making in Israeli Foreign Policy: An Unplanned Process

LEWIS BROWNSTEIN

The October 1973 war has brought many changes to Israeli society. The losses in lives, material, and international support have led to a major economic crisis and a heightened sense of isolation and threat. Most significantly, these losses have severely shaken the confidence of the people in the future of the society itself, in its ability to survive over the long run. This loss of confidence has been manifested in many ways (a series of crippling labor disputes, open conflicts over settlement in the occupied territories, a more bitter and divisive style of political debate both within the ruling Labor alignment and between the alignment and the Likud opposition), but especially in a precipitous decline in the stature and prestige of the top government leadership, arrested to some extent by the recent successes of the Entebbe raid and the open frontier with southern Lebanon.

It is not simply that the leadership is blamed for the mistakes which led to the reverses of the war (although that is part of it). Rather, there is a sense that the war revealed some fundamental weaknesses in the structure, functioning, and value orientation of the society, perceived by some before the war, which could not be ignored after it. The result of this changed perception has been an intense reexamination of many aspects of the society, including its goals and the way in which its decisions are made. These have been subjects of debate in the press, over television and radio, in the academic world, and within the government

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itself. Of all the areas of the society which have been thus scrutinized, only the economic system has received more attention than the area of foreign policy.

Since the October war, foreign policy has become in Israel the subject of continual and acrimonious debate. The authority of the prime minister has been challenged in this area in a way that is unprecedented. In the past, Israelis tended to defer to their prime minister in decisions regarding foreign policy and security. From the beginning of the state there had been a basic consensus in the country on these matters. This consensus, which concerned the nature of the threat facing the society, if not always the tactics to be followed in dealing with it, gave the leadership virtual freedom of action.[1] It freed them from the constraints normally imposed by the fractured nature of Israeli political life, which placed more severe limits on their actions on domestic issues.

Since the war, the old consensus on foreign policy matters has dissolved. The country is facing a series of painful choices and there is no clear idea of what policies it ought to adopt. This is compounded by the fact that the present ruling triumverate (Prime Minister Yitzhak Rabin, Foreign Minister Yigal Allon, and Defense Minister Shimon Peres) is the weakest in terms of public support in the history of the state.

The debate over the future of Israeli foreign policy has, quite naturally, been focused primarily on the policies Israel ought to adopt vis-à-vis her Arab neighbors and the occupied territories. Increasingly, however, attention has also been directed toward the mechanisms by which policy choices are made. There has been a growing suspicion that some of the errors of the past might have been avoided had more systematic and rational policy-making procedures been employed. This opinion is held by only a minority in and out of the government, but a variety of factors have been at work which have had the effect of increasing support for this position.

Since the establishment of the state in 1948, Israeli foreign policy decision making has tended to be highly personalized, politicized, reactive, ad hoc, and unsystematic. This article focuses on four prominent features of foreign policy making in Israel:[2] (1) domination of foreign policy making by a small elite; (2) the lack of planning in decision making; (3) the underdevelopment of in-house research capability in foreign-policy institutions; (4) a resistance to the utilization of outside expertise in foreign policy making. While highlighting changes that have been introduced since the October war, the article describes the system as it has evolved and examines the chances for the adoption of more systematic approaches to decision making.

[1] See Peter Y. Medding, *Mapai in Israel: Political Organization and Government in a New Society* (London, 1972), pp. 214–215.

[2] This is not the place for a detailed discussion of the Israeli political system itself. In addition to ibid., readers might wish to consult: Leonard Fein, *Israel: Politics and People* (Boston, 1967); and relevant chapters of Nadav Safran, *The United States and Israel* (Cambridge, Mass., 1963) and S. N. Eisenstadt, *Israeli Society* (London, 1967).

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It should be emphasized that no attempt is being made here to evaluate specific policies of the Israeli government either before or after the October war. There is a vast literature on that subject already. Nor does this article aim to suggest what policies ought to be adopted.

Since the October war, there has been an increased willingness on the part of some Israeli policy makers, especially Allon and Peres, to experiment with the use of planning techniques, particularly short-term contingency planning, as well as to utilize more expertise in policy making. Aside from the trauma that followed the war, the impetus for these changes has come from such factors as the example that is being set in other ministries (especially the Ministry of the Treasury); the increasing availability of trained talent in the field of foreign and defense affairs; the growing recognition that as Israeli society becomes more complex, decision making must be rationalized even in the field of foreign affairs; the impact of the example set by Kissinger in the interim negotiations; and, perhaps most significant, the competition among the three leaders themselves in which each has attempted to use his ministry as a substitute for the power base he lacks outside the government.[3] Despite these changes, however, the essential nature of the Israeli decision-making system remains what the early leaders of the state made it. It remains unclear whether the recent innovations will take root and become institutionalized or whether the deep-seated resistance to such techniques will reassert itself. The actions of Rabin himself indicate at best an ambivalence toward planning and systematization.

Domination by a Small Elite

Foreign affairs has, from the beginning of the Israeli state, been controlled by a very small group of people.[4] Deriving their power from leadership positions in Israel's main coalition parties or being protégés of such leaders, this group has made the strategic and tactical decisions[5] that have guided foreign policy. The key individual in the Israeli government has always been the prime minister. More than any other member of the high policy elite, the prime minister has

[3] This is the first time in the history of the state that the top leaders were not members of *Mapai* before assuming their posts. Rabin has no political base, his previous experience being restricted to a stint as chief of staff of the Israel Defense Forces (1963–1967) and as Israeli ambassador to the United States for four years under Golda Meir. Peres was a protégé of Ben-Gurion, director of the Defense Ministry, and a member of *Rafi*, a breakaway party from *Mapai* led by Ben-Gurion. Allon, a leader of *Achdut Ha'Avodah*, had perhaps the closest ties to the old elite, having been a member of the earlier cabinets and close to Meir.

[4] Michael Brecher in his essential book *The Foreign Policy System of Israel: Setting, Images, Process* (New Haven, Conn., 1972) identifies just eighteen people in Israël's "High Policy Elite" for the years 1948–1968 (see Table 22, p. 221). It will be noted that of these, at least four were significant in issue areas (economics and culture) which were not directly related to foreign affairs as such.

[5] For a discussion of the distinction between these two types of decisions see ibid., pp. 373–378.

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determined the direction of Israeli foreign policy. The degree of the prime minister's dominance has depended upon such factors as each one's personality, the presence or absence of other individuals in a position to challenge this supremacy, the degree of cohesion of the society's elite, and the willingness of other political leaders and the public to defer to the prime minister on foreign and security matters.

It was David Ben-Gurion, the first prime minister of the state (1948–1953, 1955–1963) who established the preeminence of this position in security and foreign affairs. From the beginning he insisted that he alone had to have direct control in these areas. To assure this, he was even prepared to sacrifice some of his authority in other areas, notably economics.[6] It was his refusal to share his power in foreign affairs that led to his split with Moshe Sharett, the first foreign minister, in which Sharett was finally forced to resign.[7]

While no other prime minister has enjoyed the degree of independence of Ben-Gurion, he set a precedent that has been followed more or less faithfully by his successors. Thus, Levi Eshkol (prime minister from 1963–1968), whose style was to consult fully with many people in and out of his cabinet before making a decision, nonetheless kept the reins of power in his hands at least until the days just before the 1967 war. Golda Meir (prime minister from 1968–1974), who had been dominated by Ben-Gurion when she was a foreign minister, was known for keeping her own counsel and consulting very few people. Even Rabin, who came to power in a cabinet characterized by the first real internal division of power, has been able to emerge as the *primus inter pares* in the field of foreign policy.

Prime ministers have not been able to make foreign and defense policy in isolation from other members of the political system, however. In that system, the prime minister occupies the center point within three (analytically defined) concentric circles: an informal circle of close advisors sometimes referred to as the "Kitchen Cabinet"; a circle composed of the heads of key power blocs within the Labor party; and an ad hoc somewhat amorphous collection of individuals who gain access because of their relationship to members of the other two circles. The goal of the prime ministers of Israel, especially after Ben-Gurion, has been to maintain their freedom of action in foreign and defense affairs by assuring themselves of support, either tacit or overt, from the power blocs represented within these circles, building consensus while at the same time restricting access to the smallest possible group. Failure to obtain this support can lead to a serious deterioration in the prime minister's power, something that Rabin has discovered since taking office.[8]

[6] See Medding, *Mapai in Israel*, p. 215.

[7] The conflict between the two is discussed at some length in Brecher, *Foreign Policy System*, pp. 379–391. See also, Medding, *Mapai in Israel*, pp. 215–217.

[8] See Yosef Goell, "Labour Seeks a Cure," *Jerusalem Post* (Weekly Overseas Edition) March 23, 1976, pp. 9–10. In March 1976, in an unprecedented move reflecting a precipitous loss of

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The "Kitchen" is a term that has come to stand for a small group at the center composed of the prime minister and his closest advisors which makes policy primarily on foreign and security affairs. Although the term was in use during Ben-Gurion's time, the Kitchen system can be dated from Eshkol's loss of power and prestige just before the June 1967 war when he was forced to turn over the defense portfolio to Dayan. This had the effect of opening up decision making in foreign and defense affairs to a wider group within the elite. Henceforth, the prime minister was going to have to share some of his power. The Kitchen system, which was particularly associated with Golda Meir's tenure, was an attempt to be responsive to the pressures of the coalition while at the same time shielding top policy makers from it. Having no statutory status, the Kitchen became both the embodiment and the symbol of the informal, personal, and unsystematic nature of Israeli foreign policy making. As such, it came in for a substantial amount of criticism, especially after the October war, both from members of the coalition not in the Kitchen who resented being excluded from its meetings, as well from commentators and analysts in the society at large.

The specific membership and style of the innermost circle has varied according to the personality and stature of the prime minister. In Meir's time, the circle was informal, ad hoc, but nonetheless authoritative, because of Meir's status in the Labor party and the society at large. Aside from the prime minister herself, it included Dayan, Allon, who was then deputy prime minister, Yisrael Galili who was minister without portfolio and a close personal advisor to Meir, and Pinhas Sapir, the finance minister. This core group would from time to time bring in others such as the chief of staff of the Israel Defense Forces or the foreign minister.

Under Rabin, the style has been different. In place of the Kitchen, there has been the "negotiating team" composed of Rabin, Allon, Peres, and their close advisors. It has met relatively infrequently, has been marked by sharp internal division, and perhaps most significantly, has lacked the authority which was possessed by its erstwhile counterparts. Instead of attempting to coordinate overall foreign and defense policy, even if informally, Rabin has attempted to isolate himself from pressure emanating either from within the ruling coalition or from outside. In part this is a matter of personality and in part it is a reflection of his weak political base.[9] He has surrounded himself with a number of key advisors who owe him allegiance in what appears to be an attempt to compensate for his weak position within the Labor party.[10]

The second circle consists of the top leadership of the Labor party. Sometimes referred to as the *Havereinu*, it is drawn from the Labor ministers of the cabinet

confidence in Rabin's leadership, the party leaders brought Golda Meir out of retirement to head up a new "Steering Forum."

[9] See ibid., and David Landau and Malka Rabinowitz, "The Rabin Enigma," *Jerusalem Post*, December 23, 1975, pp. 8–9.

[10] See Y. Ben-Porat, "All Rabin's Advisors," *Yediot Ahranot*, July 25, 1975, pp. 5–10.

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(*Sareinu*), the leaders of the main factions in the party,[11] and the top leaders of the *histadrut* (General Federation of Labor). The *Havereinu* had its origins in the 1950s.[12] For many years it was the more or less informal consultation group within the party. It was within this group that many issues of domestic and foreign policy were debated and hammered out before being presented to the full cabinet. While the *Havereinu* were more influential on matters of domestic policy, the prime minister needed the support of this group on foreign-policy issues as well. This was usually forthcoming and with it he or she could then be assured of control over cabinet decisions.

So long as there was a basic consensus within the Labor party, the prime ministers of Israel were able to insulate themselves from further outside pressures on foreign and defense affairs. Since the October war, however, the weakening of that consensus within the leadership has led to a decline in the organization and cohesion of the *Havereinu*. Instead of uniting behind the prime minister, once the internal debates have occurred, the leadership has been fractured into a number of competing factions. One of the consequences of this has been to create the conditions allowing for more penetration by individuals in the third circle. Lacking a power base derived from the party, each of the three members of the triumverate has had to go outside the formerly close-knit elite to find allies and support. They have brought outsiders into their ministries, held informal discussions with individuals from many walks of life, and relied more heavily on their top civil servants.

The specific membership in this third circle shifts fairly often. Aside from those in the ministries, it tends to be an amorphous group to which individuals gain access because they possess political influence or occupy a key bureaucratic post, or have close personal ties to one or more of the people in the first two circles. One result of the expansion of this third circle has been to indirectly involve many more people from Israeli society in informal consultation on foreign affairs. Since these interactions do tend to be informal, ad hoc, and private, however, it is difficult to assess their precise significance in terms of actual policy making. Nonetheless, it would appear that the impact of this circle has been marginal at best.

In spite of internal divisions within the Israeli political system, control over policy is still in the hands of the top elite. Rabin, although he is the weakest prime minister in Israeli history, retains primary control over the making of foreign policy. He is aided by many of the same factors that tend to strengthen the dominance of the executive in this area in other societies: the need for secrecy and speed in decision making, the complexity of the subject matter especially when it concerns security issues, the general tendency of people everywhere to give the benefit of the doubt to their leaders on forign-policy questions.

[11] The Israel Labour party was formed in 1968 in a merger of *Mapai*, *Rafi*, and *Achdut Ha'Avodah*. These factions continue to function as competing power blocs within the party.

[12] See Brecher, *Foreign Policy System*, pp. 426–429; and Medding, *Mapai in Israel*, pp. 122–124.

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What the current disarray in the leadership does is to severely restrict Rabin's capacity to plan and to take initiatives in foreign policy even if he wished to do so. Faced with a host of conflicting demands, his strategy has been to eschew actions that threaten to alienate any of the constituent elements of his shaky coalition. At the same time, he has turned inward in order to shield himself from unwanted pressures. Consequently, the individualistic, ad hoc, reactive, and unsystematic tendencies of Israeli foreign policy making has been accentuated.

Lack of Planning in Foreign Policy Decision Making

Israeli decision making in almost all areas (with the possible exceptions of agriculture and defense) is characterized by a strong bias against planning.[13] There is a resistance to anticipation, initiation, or evaluation. This is especially true in the field of foreign affairs. There was virtual consensus in interviews conducted with members of the academic community, government officials, and politicians that Israeli foreign policy is almost entirely reactive in nature, taken up with ad hoc "fire brigade" activities rather than attempts at long-, medium-, or even short-range planning.[14] Since the October war, some attempts have been made at "contingency planning," but these have been primarily focused on situations in which the Israelis were responding to someone else's initiatives. (For example, there was reportedly some such planning in response to the threat to expel Israel from the United Nations in fall 1975.)

The consensus just mentioned broke down over the possibility or desirability of changing the present situation. While many observers bemoan the absence of planning in fields like economics of urban development, there is less agreement as to the feasibility of planning or systematic process in the making of foreign policy.[15] Particularly among members of the Israeli government there was a feeling that Israel is not in a position to plan her foreign policy. This is because she is a small state fighting for her survival in a hostile environment in which her actions must necessarily be responses to the initiatives of her neighbors. Since her foreign policy must be reactive, there is little point in drawing up plans whose implementation is problematic at best. Events such as the Yom Kippur war or the severing of relations by the Africans have only reinforced this view. One Foreign Ministry official discussing the latter event said: "I have thought this over many times and I am convinced that there is nothing we could

[13] A good summary of the reasons for this can be found in Benjamin Akzin and Yehezkel Dror, *Israel: High-Pressure Planning* (Syracuse, N. Y., 1966), especially chap. I. See also Dan Inbar and Gabriel Sheffer, "Environmental Influences on Changes in Public Policy-Making in a Small State: The Case of Israel," mimeo., Jerusalem, September 1974.

[14] This agrees with Brecher's findings as well. See *Foreign Policy System*, p. 18.

[15] Akzin and Dror, strong proponents of planning, do not include foreign policy in their discussion of areas that should be planned. However, Dror does include "Relations between small states" as a policy issue that might be researched in an "Institute for Policy Analysis" which he recommended. See his "An Israeli Institute for Policy Analysis," *Public Administration in Israel and Abroad 1967*, An Annual Collection of Articles, 8 (Jerusalem, 1968), p. 162.

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have done which would have materially changed the result."[16] This feeling, that Israel is at the mercy of forces which she can do little to control, expressed many times in interviews with people in government, is a major contributing factor to the antiplanning bias in foreign policy.

Another factor that tends to act as a deterrent to planning is the lack of political consensus on some of the major choices facing the society. This is exemplified in the conflict over the future of the West Bank and other occupied territories.[17] Working with a bare majority in the Knesset (approximately 64 out of 120) the Rabin government risks bringing itself down if it comes up with an explicit plan for the areas. This is because it depends for its continued tenure on the support of both *Mapam* (which opposes retention of the West Bank, for example) and the National Religious party (which favors retaining the West Bank and supports Jewish settlement in the area). Under these circumstances, the government is unable even to initiate extensive internal discussions on certain issues much less come up with overall plans for their disposition. Many issues in Israeli foreign policy are of this nature.

A third factor that makes planning less likely concerns a pragmatic emphasis on tactics rather than strategy which characterized political life in Israel even before the establishment of the state.[18] This factor works against planning because the assumptions of planning are different than those of politics. Planning assumes the possibility of rationality and objectivity. It assumes that a variety of options are available and that the task is first to identify them and then to choose the one which maximizes the goals the government is trying to achieve. The politicized system of decision making in Israel[19] starts from the assumption that rationality, especially as understood by the academic analyst, is simply not possible. As one senior official in the prime minister's office said in an interview: "We do not have many options. We are happy when we have one and one-half options."[20] The result of political pressures is often either to foreclose options or to force the adoption of some course of action that falls far short of being rational.

The emphasis on tactics rather than strategy can be traced back to the earliest

[16] Interview in Jerusalem, May 1975. (A condition of the interviews conducted for this study was that the identity of the respondents would be kept confidential. For that reason, no names are used.)

[17] See, for example, Naomi Shepherd, "Jerusalem's Settlement Policy Is a Tool for Diplomacy," *The New York Times*, April 4, 1976, Section 4, p. 1.

[18] See Akzin and Dror, *Israel*, p. 2, and Daniel Segre, *Israel: A Society in Transition* (New York, 1971), p. 39.

[19] But not only in Israel. An extensive literature has developed on the political nature of decision making in the United States and elsewhere. Some people who have written on this include Roger Hilsman, Charles E. Lindblom, and Morton H. Halperin. On the problems of applying a rational approach in decision making, see Sidney Verba, "Assumptions of Rationality and Non-Rationality in Models of the International System," reprinted in James N. Rosenau (ed.), *International Politics and Foreign Policy*, rev. ed., (New York, 1969), pp. 217–231.

[20] Interview in Jerusalem, April 1975.

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days of the state and even beyond. Especially in the years immediately following the War of Independence (1948–1949), Israeli leaders found themselves facing an unprecedented situation both internally and externally. Surrounded by implacable enemies, faced with the absorption of refugee populations equal to more than 100 percent of the total Jewish population in 1948, struggling to overcome the effects of the war which had seriously disrupted the country's economy, there was simply no time to plan and virtually no blueprints to follow. Improvisation was the rule because it was the only choice. There can be no question that the memory of those years, and of the monumental successes that were achieved in a situation of supreme adversity, resulted in a collective memory on the part of the leadership. This experience only reinforced their conviction that problems should be dealt with as they arise without wasting time on fruitless speculations about alternatives which in reality do not exist.

The experience of those years tended to instill a deep suspicion of experts, especially in the fields of social sciences and humanities. Forced by circumstances to act, and possessing no guidelines, Israel's leaders had little patience with people who kept telling them why they must fail. Ben-Gurion, who personified this attitude, said in an interview dealing with his years in power:

> The expert must know his facts and figures in the field in which he has specialized. He does not have to know people, and in the nature of things, having devoted his time to his specialist subject, he is unlikely to have had much experience in political organization or leadership. A Prime Minister, on the other hand, has got there precisely through such experience—an experience which enables him to know people, to know their capacities, to know their responses to a particular policy in a particular circumstance. If, allied to his judgement of people, he can also make a wise assessment of situations, he can go ahead and fix his priorities with confidence no matter what the experts say.[21]

The successes achieved in the early years instilled in the leadership a deep conviction in their abilities and in the quality of their judgments. It would be difficult to overemphasize the influence of those years or their effect on the pattern of Israel's decision making in foreign policy. Indeed, of all the fields with which the leaders were concerned, foreign affairs, in their view, was the most dependent on judgment and intuition, and the least amenable to planning, rationality, or systematization.

The bias against planning or systematization has, it should be pointed out, come under increasing attack particularly since the October war. A number of people in government, though still a minority, have come around to the view that even if Israel cannot plan her foreign policy over a period of years, she can do much more to marshall whatever assets she possesses over the short-run. It is this belief which, in part, led to the establishment of the "Planning Unit" in the army as well as the "Research and Planning Center" in the Foreign Ministry. In both these units there is a much greater emphasis on the gathering and dissemi-

[21] *Ben-Gurion Looks Back* (in talks with Moshe Pearlman) (New York, 1970), p. 122.

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nation of information than there is on planning as such. The main reason for establishing the center, for example, was to pluralize the intelligence function in the government. Nonetheless, the very fact that the word "planning" appears in the name of each, suggests that a change of perception as to the ultimate utility of achieving at least a short-term planning capability has occurred among some people in the government. A common complaint expressed in Israel's press by journalists, academics, and government people alike is that Israel is too reactive. Her failure to think ahead has resulted, on at least one occasion, in a major military and diplomatic setback.[22]

This remains a minority position within the government and probably the society at large. Whatever their critics may say, the present leadership, and especially the prime minister, remain highly skeptical as to the utility of planning units. While the first tentative steps toward the creation of planning capability may have been taken in the foreign and defense ministries, Rabin has resisted suggestions that he establish a coordinating body on foreign and defense policy in his office.[23] In addition, the Prime Minister's Office has been characterized by an almost total lack of staff work or even systematic gathering of information on key foreign policy issues. There has been very little if any coordination between this office and the Foreign and Defense Ministries. In short, Rabin insists on relying on his own judgment. He defends his approach by arguing that coordinating bodies and systematization would only reduce his flexibility while adding little or nothing to the quality of decision making. Caught in a web of conflicting domestic and foreign pressures, he prefers to shield himself from them, keep his own counsel, and thereby preserve as much freedom to maneuver as possible. He opposes establishing structures which threaten to tie his hands at a time when he feels he must be free to improvise.

That his attitude is more a result of his institutional role than of his personality alone is suggested by an examination of his style when he was chief of staff of the Israeli army. In interviews with people who worked with him as well as published accounts of his performance in that job, it is clear that Rabin was highly organized, placed a premium on careful planning, and was responsible for introducing modern management techniques into the Israeli army.[24] As one of Rabin's former military colleagues put it: "Many of us expected him to open the system up, to consult with people widely as he did when he was Chief of

[22] Thus, Israel's failure to consider the possibility of Russian intervention in response to her deep penetration raids starting in January 1970 against Egypt resulted in a confrontation between the two states in which Israel was forced to exchange her dominance for something approaching parity in the Canal Zone. See the reports of Golda Meir's interview with Amos Elon in *Ha'aretz* July 24, 1970, which appears in Edward Luttwak and Dan Horowitz, *The Israeli Army* (London, 1975), p. 323; also, Lawrence L. Whetten, *The Canal War: Four Power Conflict in the Middle East* (Cambridge, Mass., 1974), pp. 89–122.

[23] He stated his position very forcefully on *Moked* (Israeli TV) May 10, 1975, and repeated it in a discussion before the Hebrew University faculty on May 19, 1975.

[24] See, for example, Luttwak and Horowitz, *The Israeli Army*, p. 181.

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Staff. Instead he has conformed to the system. He has changed nothing. His impact is nothing on the political system. He keeps out of all associations."[25] The disappointment expressed in this comment is typical of those who had hoped that Rabin, since he came from outside the old elite and was used to planning and wide-ranging consultation, would behave differently from leaders of the past. Instead they found he adopted the modes of his predecessors. As one sympathetic official of the Foreign Ministry commented: "I guess he found that he had to play by new rules. In the Army he was in a closed system protected from criticism. Now he is a leader of a coalition government."[26]

Underdevelopment of In-House Research Capability in Foreign Policy Institutions

One of the reasons for a failure to anticipate or evaluate (as well as a consequence of not doing so) is that Israel's foreign policy institutions have simply not been in a position to support such activities. Of the four institutions directly concerned with foreign policy formulation—the Office of the Prime Minister (which we have already discussed), the Foreign Ministry, the Knesset, and the Defense Ministry—only the latter has had a substantial in-house research capability. Even in this case, production of in-depth studies of foreign policy relevance has been an adjunct activity.

It should be emphasized that it is not the intent of this article to suggest that no planning or systematic policy making is occurring in Israeli ministries. As already pointed out, there have been increased efforts at contingency planning and a greater willingness to experiment with projection techniques such as simulation than was the case before the October war. Thus, contingency planning was employed in the context of the negotiations over the second interim agreement with Egypt particularly by the Army's Planning Unit and a task force was formed in the Foreign Ministry in the fall of 1975 to plan for the eventuality of Israel's expulsion from the United Nations. Nonetheless, these efforts remain exceptions to the rule and have not altered the basic nature of foreign policy decision making in Israel.

Foreign Ministry

The problem with the Foreign Ministry concerns personnel as well as a failure to develop institutional capabilities. With respect to personnel, the ministry finds itself burdened with a large number of people, taken on in the early years of the state, who lack substantial qualifications. We are not refering here to what Brecher calls the "Foreign Service Technical Elite" (FSTE),[27] people who hold the

[25] Interview in Jerusalem, May 1975.
[26] Interview in Jerusalem, April 1975.
[27] Brecher, *Foreign Policy System*, chap. 17.

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post of department head or higher, but rather to those below this level. In the early years the educational qualifications for such positions were relatively low. Under the system of bureaucratic seniority, however, they cannot be replaced until they retire.

Many times in the course of interviews with members of the FSTE complaints were expressed about the degree to which these people retard changes in the ministry. The following comment by the head of a key geographical department was typical: "We have a very serious problem of deadwood in this ministry. In the early years we expanded very fast and we couldn't be very choosy. These people were not able to rise in the hierarchy. They are o.k. for second- and third-rate jobs. Not the first-rate jobs. We just can't get rid of them. We are building up a cadre of graduates and by 1980 I would expect we would be able to function more systematically."[28]

The consequence of this situation is that the top personnel of the ministry cannot rely on their junior staff. They are forced to produce papers on their own, very often relying on their own expertise while working under the pressure of the moment. The quality of the final product depends very much on the degree of expertise the man on the spot brings to the question in hand.

Until the spring of 1975, the establishment of the Center for Research and Planning in the ministry had not developed much capability to support systematic policy making, nor had it been encouraged to do so. As one top member of the FSTE put it about this time: "The Foreign Ministry has no role in policy-making. The Minister represents himself in the inner circle. There is no articulated view of the ministry. The present Minister is very much a man of himself. Unlike other ministries, such as Social Welfare, Defense, or Health, where the role of experts is much more significant, this Minister is able to operate on his own. And he does."[29] This particular man went on to speak about the demoralization which had resulted from this failure to utilize the expertise which does exist as well as the failure to encourage the development of an overall approach to policy making.

Historically, the ministry has been responsible for the day-to-day activities of Israeli foreign policy—what Brecher has called the "implementing decisions."[30] It has lacked an organized in-house research capability or even a uniform filing system or system of information retrieval. Operational departments have had access to only limited library facilities of their own and often functioned without experts in their areas of concern. Indeed, people were moved around from one department to another so often that there was little opportunity for them to acquire expertise. In the spring of 1975, for example, the ministry had two people

[28] *Interview in Jerusalem, April 1975.*

[29] Interview in Jerusalem, May 1975.

[30] Brecher, *Foreign Policy System*, pp. 520–541, for a detailed discussion of the Foreign Ministry's activities.

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fluent in Chinese, trained at the ministry's expense, neither of whom were working for the Asia Department. Rather than an ongoing procedure of information collection and analysis, coupled with systematic examination of policy options, problems have been dealt with as they arise with papers produced, if at all,[31] only in the context of a "crisis" or an immediate policy problem. Operational department heads have not been expected or encouraged to suggest policy alternatives. This has not necessarily prevented them from doing so from time to time. The point is that the system has been totally dependent on the individuals composing it. No organized rationalized process of data gathering and analysis has existed.

The lack of a well-developed system of information analysis within the Foreign Ministry has led to its becoming a stepchild in Israeli foreign policy. As a ministry, it has lacked substantial influence. Its problems have been aggravated by the prevailing bias against planning and systematization, by the highly personalized style of decision making, and by the domination of Israeli prime ministers over foreign policy.

Allon, in an attempt to increase his influence with the Rabin administration[32] has taken a number of steps to strengthen his ministry. His apparent goal has been to develop a capacity within the ministry for information gathering and analysis which will make it possible for his voice to carry greater weight in policy making as he competes with Rabin and Peres. In this, he is breaking with previous patterns. With the possible exception of the Defense Ministry[33] bureaucracies in Israel have not provided ministers with power bases. As a consequence, there has been no political incentive for ministers to develop their ministries' strength. This was especially true for the Foreign Ministry. The steps Allon has taken have included the establishment of the center and the appointment of a leading member of the academic community, Shlomo Avineri (dean of Social Sciences, Hebrew University) to the post of director-general of the ministry.

The center had its origins in the criticism of the Intelligence Unit of the Army after the October war. The Agranat Commission, which was formed to evaluate the events leading up to the war, came to the conclusion that the government needed to pluralize its intelligence function.[34] Since its formation in early 1975, the center has built up a large staff (about sixty by the spring of 1976) and its director (who was brought over from the Defense Ministry) has appeared a number of times before the cabinet to brief it on intelligence matters, a role formerly

[31] Israel is very much an "oral" society. Papers, when produced, are rarely read.

[32] Allon was much more influential during Meir's time than he has been since Rabin became prime minister. Under Meir he was deputy prime minister and a frequent participant in the "Kitchen."

[33] Dayan and Peres have derived substantial power from control of the Defense Ministry while Ben-Gurion and Eshkol were greatly strengthened by holding that portfolio.

[34] The commission produced three reports. Only the first, published April 1, 1974, was made public, the other two being submitted July 10, 1974, and January 30, 1975.

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reserved exclusively for the director of Army Intelligence. The center has also been providing services to departments within the ministry, disseminating information, and acting as a clearing house. While most of the work of the center is done by regular ministry personnel, academics from Hebrew and Tel Aviv Universities have been called upon from time to time. In particular, the center has been able to draw on the talents of a number of young scholars, most of whom were recently trained in the United States. These scholars, whose specialties include Soviet, American, and Arab area studies as well as strategic issues, have been used as consultants, called in for combined group discussions with center personnel, or, in some cases, asked to prepare short, well-defined studies as part of some overall project on which the center was working.

The appointment of Avineri was apparently motivated by two considerations. First, he has been a vocal critic of government foreign policy, especially as it has related to the Palestinian question. On this, his views have been close to those of Allon. (Both men favor a stated willingness to speak with any Palestinians who will accept Israel's right to exist.) By bringing him into his ministry, Allon obtains an articulate supporter. Second, Avineri is committed to building up the ministry and possesses the administrative skills to do the job.

In spite of these developments, it is not possible to say that the Foreign Ministry is in the process of developing a capacity to support systematic policy making; nor is it clear that the influence of the Foreign Ministry will be substantially increased by these changes. In the first place, the center is still primarily involved in preparing intelligence estimates for the government. There is no evidence either that it has been engaged in attempts to develop long- or middle-range planning or that it has produced recommendations which have had impact on the government's policy. Indeed, this was not the purpose for which it was created, as a reading of the Agranat Commission's recommendations show. To the extent that its intelligence estimates are accurate, it may strengthen Allon (this has not happened as of this writing) but this would not by itself constitute a rationalization or systematization of the policy process. Second, there are substantial structural obstacles to a fundamental change in the role of the ministry: the presence in the ministry of large numbers of people (some of whom are intensely jealous of the center) who resist the changes which are occurring; the essentially political nature of the issues on which policy must be made; the resistance of the prime minister himself. One suspects that the Foreign Ministry will not prove to be an effective power base. Finally, Allon's decision to appoint Avineri ran into substantial resistance both from inside and outside the Labor party. For the first time, the appointment of a director-general was the subject of intense debate within the cabinet. This only points up the fact that decision making in Israel remains a highly personal matter. In short, it is by no means clear that Allon's innovations, limited as they are, will continue once he ceases to be Foreign Minister.

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The Knesset

Israel's law-making body also lacks effective research capability in the foreign affairs area. The library, which serves the entire Knesset, has a staff of two or three full-time people and perhaps half a dozen part-time postgraduate students from Hebrew University who are hired for a year at a time. These latter are positively discouraged from making library work in the Knesset their career. No one on the library staff has specific training in foreign affairs.

The Knesset Committee on Foreign Affairs and Security, the key legislative committee dealing with foreign policy matters,[35] has one full-time staff member who is on loan from the Foreign Ministry. The committee lacks any independent investigative capability, nor does it concern itself with retrospective evaluations of Israeli policy on a regular basis. Its members are much too caught up in following the day-to-day events. Its main function is to provide a liaison between the Knesset and the government as well as an opportunity for Knesset members to question leading government personnel on current policy. Members of the Knesset Committee defended the absence of an independent information capability as both proper and inevitable in parliamentary democracy based on a system of government responsibility. Whatever one may think of that point of view, it is clear that the committee, which counts among its members some of the most knowledgeable and influential Israeli politicians on foreign affairs, is totally dependent on the government's representatives for its information. Partly as a consequence of this, the government may liaise with the committee, it may even consult with it, but it is not likely to be seriously challenged by it.

Defense Ministry

Only the Defense Ministry and the army have possessed an independent research capability in the area of foreign affairs.[36] The significance of this can be exaggerated, however. Its nature is materially affected by the ministry within which it is located. This can be demonstrated by examining four points.

First, the main concern of the Defense Ministry and the army is necessarily with security, with a heavy emphasis on Israel's military potential vis-à-vis her Arab neighbors. There can be no question but that Israel has invested substantial sums on research in this area. Most of this has been devoted to strictly military research—e.g., weapons development—and to intelligence with some directed to a study of politically relevant issues. While there are no public figures on the

[35] See Brecher, *Foreign Policy System*, pp. 128–133, for a discussion of the role and significance of this committee.

[36] For the purpose of this discussion, there is no need to distinguish between the Defense Ministry and the army. Especially under Dayan, the distinction became blurred. See Chaim Herzog, *The War of Atonement* (Jerusalem, 1975), p. 41.

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proportion of defense funds, especially intelligence funds, devoted to foreign policy research, it is clear that the relative amounts are small and that whatever is being done is designed to supplement the security- and intelligence-related interests of these two organizations. From the earliest days of the state, foreign policy has been subordinated to security considerations.[37] Defense has been the dominant ministry in Israeli foreign policy[38] with inevitable results for the way issues and priorities have been defined. While the Defense Ministry and the army may, in the course of their major activities, conduct research or collect data of foreign policy relevance, these latter activities are secondary to their main concerns.

Second, the utilization of such information and analysis has been affected by the security prism through which it has been refracted. As the dominant ministry in foreign affairs, the information amassed by Defense and the army has tended to buttress the trend of placing primary emphasis on security matters. This, in turn, has further weakened the position of the Foreign Ministry, particularly in confrontations over policy at the highest levels, thus closing the circle. In other words, even though the Defense Ministry and the army may be carrying out research on foreign policy questions, they are doing so for their own reasons and are not thereby filling the lacunae existing in the other institutions of the Israeli government. The topics that the Defense Ministry chooses to examine are different from those the Foreign Ministry would study if it were carrying on extensive research and planning on foreign policy issues.

Third, the Defense Ministry, and particularly the Intelligence Unit, has syphoned off most of the available talent in the foreign policy area. This is not surprising given the ministry's prestige, but the result has been to make it more difficult for other ministries, particularly the Foreign Ministry, to recruit experts even on a part-time basis. Because the Defense Ministry is so much more prestigious, people are more anxious to work for it since in this way they have a better chance of making a significant contribution.

Finally, and perhaps most important, there is a real question as to whether even that information which is being gathered and those studies which are being conducted are finding their way to the high-policy levels of the government. From interviews with people in Israeli academic institutions doing such work for the Defense Ministry and the army as well as people in the government itself, there emerges a strong suspicion that much of this effort is wasted, the result, perhaps, of an inflated budget rather than a concerted effort at foreign policy analysis. Given the size of the Defense Ministry's budget, it has been in a position to finance work by its staff in a variety of fields. Much of this work does move up the ladder within the ministry, but its impact is probably not substantial.

[37] See Brecher, *Foreign Policy System*, pp. 381–382, on Ben Gurion's priorities in foreign policy making, and Medding, *Mapai in Israel*, p. 215.

[38] Brecher, *Foreign Policy System*, p. 398ff, which raises the question as to why Brecher devotes so little attention to it.

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Resistance to the Utilization of Outside Expertise in Foreign Policy Making

The Israeli political system has been characterized from its earliest days by an almost ubiquitous resistance to the involvement of experts in policy making. This resistance has been found at all levels of the government and has been characteristic of most fields (with some exceptions in defense and agriculture).[39] This anti-expertise bias has been directed against experts both within and outside of government. There has been a negative evaluation both of the role of the professional bureaucrat and of that expertise which might be brought in from other institutions such as the universities. Since we have already discussed the situation in the relevant ministries, we will now focus on the relationship between the government and the universities in the field of foreign policy. Aside from the government itself, the universities contain the reservoir of talent on which the government would have to draw if a more systematized approach to foreign policy were to be adopted.

Stated briefly, there is a virtual absence of institutionalized links between the government and the academic environment in the area of foreign policy. Such interaction as there is tends to be ad hoc, poorly planned, and almost never on "neutral" ground. Israel as no independent "think tanks" or councils where academics and government officials can come together to exchange views. The government has no policy planning councils of its own which might serve the same purpose, nor is there a pattern of "horizontal" movement from academic life to government or vice versa.

Before the October war, whatever interaction there was took the form mainly of informal contacts during social gatherings—the famous "Friday night" parties.[40] Since the war, a number of efforts have been made, none of them very sustained or successful, to expand the contacts between the two groups. Thus, some of the departments in the Foreign Ministry have brought in relevant academic experts for informal off-the-record talks; organizations such as the Leonard S. Davis Institute for International Relations at Hebrew University have held conferences to which government people have been invited and have even held off-the-record talks of their own. Interviews with participants from both groups show clearly, however, that few people, if any, have found these encounters very satisfying. The only successful sustained types of contacts between academics and a government agency appear to have been those between the Intelligence Unit and some of the Orientalists and Sovietologists at Hebrew and Tel Aviv Universities. Individuals from both these groups have carried out studies for the Intelligence Unit or have consulted with it, in some cases on a regular basis. A number of the men have done their reserve duty in this unit as well and have, in some cases, been mobilized into it in times of war.

[39] Akzin and Dror, *Israel*, p. 11.

[40] This aspect of Israeli life is discussed briefly in Yuval Elizur and Eliahu Salpeter, *Who Rules Israel?* (New York, 1973), pp. 18–19.

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In addition, in something of a break with previous patterns, some professors from Hebrew and Tel Aviv Universities have been brought into the government to serve in key posts in a few of the ministries.[41] While they may succeed in establishing closer links with their erstwhile colleagues than was the case with their predecessors, the fact that two of them quit soon after assuming their posts demonstrates that academics are going to find it difficult to conform to the highly politicized nature of decision making in Israel.

Aside from the general suspicion of expertise on the part of members of the Israeli political elite which was alluded to above, there are five reasons for the lack of institutionalized links between the government and academics in the area of foreign policy. Most of them have to do with the poor relations which have existed between the two communities throughout the history of the state and the resultant low opinion which each has of the other. Of the two, however, the main resistance to strengthening the relationship seems to come from the government side. The numbers of academics who aspire to involvement in the foreign policy process have been growing all the time.

The first reason has to do with the close-knit structure of the elite itself. Israel was led to independence by a relatively small group of people who often functioned in secret. There was a strong sense of "in" and "out." Many of the leaders saw the professors as positively obstructionist and defeatest. This image was the result in part of such groups as the *Brit Shalom* of Hebrew University which had opposed the establishment of the state.[42] With the exception of certain individuals, the academic community was seen as being cut off from the political mainstream of Israeli society. They might be respected, but they were not completely trusted. This distrust has had its effects on the willingness of government leaders to involve academics in foreign policy matters. Since so much of the government's information in this area is classified, there was a reluctance to open up to a group of outsiders.

The second reason concerns a paradoxical attitude toward the academic profession in Israel. In the abstract, the "professor" is a respected figure. He is even held in certain amount of awe. This stems from the Jewish tradition of respect for learning and for men of books as well as from the influence of the German professors who came to Palestine in the 1930s during the Fifth *Aliyah*. However, there is a paradoxical disdain—even contempt—for the "intellectual" or the "ivory tower professor" who is seen, particularly by people in the government and political life, as someone cut off from the "real world," often talking about

[41] Aside from Avineri, they have included: Yuval Ne'eman, former president of Tel Aviv University who was an advisor to Peres and then quit in September 1975 over the second interim agreement with Egypt; Yehezkel Dror, professor of political science, Hebrew University, who is chief planner for the Defense Ministry; Pinhas Suzman, a former economics professor who is director-general of the Defense Ministry; and Michael Bruno, an economics professor of Hebrew University who quit as advisor to the Ministry of the Treasury in March 1976 after only six months because he felt unable to influence policy due to political pressures.

[42] See Segre, *Israel*, p. 94.

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things of which he lacks practical experience, and as someone influenced by emotion rather than hard-headed practicality. This image of the professor came out many times in the course of interviews, often without the people concerned even being aware, it seemed, of the depth of their contempt. The comment of a senior Defense Ministry official is typical of many: "Professors in Israel have never proved they know how to do. They know how to speak and to write. They have no experience. It's not like the States where you have people with experience in, say, public management. Here, they only speak. It gives them an inferiority complex."[43] Often, the attitude came out in more subtle ways. Interviews would almost always begin with a ritual bow in the direction of government-academic collaboration. As the discussion wore on, however, the person's true feelings seemed to appear. Thus, in more than 50 percent of the interviews with government people there were comments such as: "Professors should realize they don't know it all" or "I'm tired of hearing professors talk about things of which they are ignorant." Then there was the deputy director-general of the Foreign Ministry who ended the interview with the comment: "I'm sick and tired of all those think groups. It's like a cud that is continuously chewed."[44]

The third reason for an absence of links is that academics do not have any significant political weight in the Israeli political system. As one professor from Hebrew University put it: "Professors are background noise." As such, they might have to be placated from time to time, by a meeting with a top governmental official for example, but they are not significant enough to force leaders to turn to them for advice. Since the Israeli political system runs on the fuel of influence, professors are a group that can be safely ignored.

A fourth reason is the government people's conviction that there are no experts on foreign policy outside the government. Government leaders see themselves as the most knowledgeable people in the area. One of them put it this way: "What can they [professors] tell me that I don't know already? On the contrary, I often sit and listen to them talk and wish I could jump up and say: 'Wait a minute. There is something you don't know.' But I can't, so I have to just sit there."[45] This feeling, that outsiders have little of value to offer is a pervasive one at the higher levels of government. It is reinforced by the personalized and politicized system of decision making described earlier. Top policy makers and members of the FSTE feel that their perspective on foreign policy is so much more accurate and realistic than that of people outside the system that there is little to be gained, and perhaps much to be lost, by bringing them in. The only groups which are seen to have something concrete to offer are the Orientalists and Sovietologists. Here it is recognized that some members of the academic community may possess specialized knowledge which can be of benefit to the policy maker. Apart from them, there is substantial skepticism on the part of

[43] Interview in Tel Aviv, April 1975.
[44] Interview in Jerusalem, May 1975.
[45] Interview in Jerusalem, April 1975.

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government officials as to the utility of using foreign policy specialists from the academic community.

Finally, and perhaps somewhat paradoxically, there is a reluctance to bring in outsiders stemming from a basic insecurity of some government personnel, particularly in the lower echelons of the bureaucracy. Israel is a very small society in which people tend to know each other from various walks of life.[46] Many of the bureaucrats now working in the Foreign Ministry are former students of professors in Israeli universities. There is a reluctance on their part to bring in their former teachers to advise them or otherwise assist their labors. They fear it will be seen as a mark of their inability to perform their jobs or that the prestige of the people being brought in will overshadow their own, thereby threatening their status in the bureaucracy. This is a serious problem in the relations between academics and the government. It was suggested by some that this fear of outsiders was characteristic of all levels of the government. We are not convinced of this. The top leadership is not so much afraid of academics as somewhat contemptuous of them. It may be true, as some reported, that the prime minister is uncomfortable in the presence of professors. If so, he can overcome it by simply ignoring them (which is what he does by and large). The sense of threat is much more palpable in the bureaucracy.

Indications that there may be some validity to such fears were manifested in the attitudes of some academics themselves. Quite often disparaging remarks were made by members of the academic community about people in the various ministries, some of whom had been former students. These attitudes have created some real tensions in the relations between some academics and bureaucrats, thereby strengthening the resistance of the latter to bringing in outsiders.

Conclusion

It has been the contention of this article that foreign policy decision making in Israel has been characterized by a lack of planning and systematic process, that it has been dominated by a small elite, that its foreign policy institutions lack the capacity to implement a more systematic approach and that the government tends to resist input from those, particularly in academic institutions, who might be able to provide a somewhat different perspective on foreign policy issues.

Despite the changes that have taken place since the October war, it seems unlikely that things will change substantially in the near future. While more planning and systematic decision making may become the rule in some areas such as economics (although the resignation of Michael Bruno raises some questions about this,[47] such changes do not seem to be likely in the field of foreign policy.

[46] See Luttwak and Horowitz, *Israeli Army*, p. 204.

[47] See note 33.

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Here the country will probably continue to respond to circumstances within the shifting context of domestic politics. In part this is a matter of leadership, in part a question of style, habit, and structure.*

Postscript

While the election of May 17th may well result in major changes in the personnel making Israeli foreign policy, there is unlikely to be substantial changes in the way in which policy is made. If, as seems likely, Menachem Begin as the head of the Likud becomes the next prime minister, three factors will influence the policy process. First, Begin himself has a reputation for being an autocratic leader, rarely consulting those around him. In the tradition of former prime ministers he has tended to act on his own and keep his own counsel. Second, to form a cabinet, he and his party will have to negotiate policy positions with the other parties they will need in order to control a majority of the Knesset. Thus policy will be largely a function of the political compromises necessary to form a coalition. Third, the Likud has had few communication links with those individuals in the academic community who were brought into the process under the Labor Party. In short, the characteristics of foreign policy decision-making discussed in this article are likely to be accentuated in any government led by Menachem Begin.

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The Office of Management and Budget that Almost Wasn't

LARRY BERMAN

This article focuses on certain critical antecedents to the formation of the Office of Management and Budget (OMB) from the reorganization of the Bureau of the Budget (BOB) in 1970.[1] It includes an analysis of previously unstudied, remarkably rich archival material from several presidential libraries and the Records Division of the OMB on three especially interesting presidentially appointed advisory groups that recommended reorganizing the BOB: the President's Advisory Committee on Government Organization (PACGO), 1953–1961, chaired by Nelson Rockefeller; President Johnson's Task Force on Government Organization, 1967, chaired by Ben Heineman; and the President's Advisory Council on Executive Organization, 1970, chaired by Roy Ash, the group that eventually succeeded in abolishing the BOB. These groups make an interesting trio for comparison because all without exception argued that the management processes[2] in the executive office needed improvement, and that to do so the BOB

[1] Note on usage: To avoid awkwardness, I will refer to the agency by its post-1970 designation except when discussing specific historical events between its founding in 1921, installment in the newly formed EOP in 1939, and its name change to OMB as a result of Reorganization Plan No. 2 of 1970.

[2] As we will see, "management" has meant something different to each of the advisory groups. By management I refer broadly to the process whereby governmental goals are translated into action. Problems of management include the budget process, program evaluation and coordination, legislative clearance, and program implementation.

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had to be substantially reorganized or abolished. Although these groups were separated in time, professional background of their members, and presidential party, the similarity of their recommendations as evidenced by the quotations below, is striking.

> Under the Budget Bureau at present, such functions as organization and management and legislative reference have the appearance, at least, of being subordinated to budget considerations. In the proposed Office of Executive Management the perspective will be changed, and budget will come into proper focus as one important tool of management.[3]

> Creation of the Office of Management and Budget represents far more than a mere change in name for the Bureau of the Budget. It represents a basic change in concept and emphasis, reflecting the broader management needs of the office of the President. . . . Preparation of the budget as such will no longer be its dominant overriding concern. . . . The budget function is only one of several important management tools that the President must have.[4]

The archival material provides important evidence that presidential intent to alter the administrative management practices of the Executive Office of the President (EOP) through reorganization of the BOB existed for *more than three decades*. Therefore, contrary to the commonly expressed view, the 1970 reorganization of the BOB into the OMB did not result specifically from the problems of implementing the New Frontier-Great Society legislation of the 1960s,[5] or from an isolated attempt by the Nixon administration to gain control of a bureaucracy perceived as hostile to the president's programs.[6] Instead, the history of management improvement efforts in the executive office has reflected to a remarkable degree the Budget Bureau's resourceful struggle[7] to preserve its venerable post-1939 status as "first among equals" in the EOP. The first of the three groups to feel the force of these power tactics was President Eisenhower's Advisory Committee on Government Organization.

[3] "Proposed Office of Executive Management," December 23, 1958. In U. S. President, *Advisory Committee on Government Organization*, Eisenhower Library, Box 12, Folder 71. Hereafter cited as *PACGO*.

[4] Reorganization Message of President Richard Nixon to Congress, March 12, 1970.

[5] See Allen Schick, "The Budget Bureau that Was: Thoughts on the Rise, Decline, and Future of a Presidential Agency," *Law and Contemporary Problems*, 35 (Summer 1970), 519–539; Gary Bombardier, "The Managerial Function of OMB: Intergovernmental Relations as a Test Case," *Public Policy*, 23 (Summer 1975), 317–318.

[6] See Richard Nathan, *The Plot that Failed: Nixon and the Administrative Presidency* (New York, 1975); Jonathan Schell, *The Time of Illusion* (New York, 1976); Joel Aberbach and Bert Rockman, "Clashing Beliefs within the Executive Branch: The Nixon Administration Bureaucracy," *American Political Science Review*, 70 (June 1976), 456–468.

[7] See Frederick Mosher, ed., *Government Reorganizations: Cases and Commentary* (Indianapolis, 1967); Morton H. Halperin, "Why Bureaucrats Play Games," *Foreign Policy*, 2 (Spring 1971), 70–90.

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PACGO vs. BOB: "Where You Stand Depends upon Where You Sit"

For the full eight years of the Eisenhower administration (1953–1961) the President's Advisory Committee on Government Organization focused on ways to improve the administrative management processes in the executive branch. The three-man committee consisted of chairman Nelson Rockefeller, Dr. Milton Eisenhower, and Dr. Arthur Flemming.

The Management Problem

PACGO believed that the president's staff facilities for administrative management were ineffectively organized. Under the term "administrative management" PACGO grouped the full range of Budget Bureau functions: budget preparation and administration, legislative clearance and coordination, organizational improvement, management improvement and efficiency research, accounting improvement and coordination of statistical programs.

The growth of the federal government's responsibilities had "increased the volume and complexity of the President's management functions" but "an adequate, balanced organization to help the President with these expanding tasks"[8] had not evolved concomitantly. Program coordination was a major managerial problem. Because institutional arrangements that fell within the purview of a single executive department or agency did not exist, temporary staff arrangements were established in the White House to deal with these matters. The ad hoc and temporary nature of these groups meant that the president or his closest aides had to personally integrate and coordinate the program recommendations with budgetary policy.

The Office of Administration. In January 1957 PACGO made its first recommendation to the president for improving executive office management responsibilities. The president's management problems could be alleviated by establishing an Office of Administration in the EOP. The director of administration would be given broad responsibility for assisting the president in "general administrative matters in the Executive Branch."[9] The proposed Office of Administration would be located in the chain of command between the president and the (now reorganized) BOB. The director of administration would report directly to the president and be assisted by three deputies and several staff assistants from the BOB.

The budget director, at the time Percival Brundage, vehemently opposed this proposal. Brundage argued that the director of administration would create an added layer between the budget director and the president.[10]

[8] "Need for the proposed Office of Executive Management," December 23, 1958, *PACGO*, Box 12, Folder 71.

[9] Nelson Rockefeller to President Eisenhower, January 3, 1957. "Office of Administration in the Executive Office of the President," *PACGO*, Box 11, File 69.

[10] Notes of A. J. Goodpaster, "Memorandum of Conference with the President," January 10, 1957. *Whitman Diaries*, Eisenhower Library, Folder: January 1957, Diary-Staff memos.

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President Eisenhower agreed with his budget director. In a meeting with PACGO, the president said their idea "had great appeal" but "the action would tend to downgrade the top men of the Budget, of the planning office, etc., who can be obtained now largely only because they have the right to come in to the President."[11] Eisenhower instructed PACGO to develop an alternative for improving management processes in the EOP.

Assistant or Deputy Assistant to the President for Management. On October 10, 1957, Rockefeller responded to Eisenhower's request and wrote to the president that PACGO still in principle favored the reorganization of executive office management activities into an Office of Administration "so that you can rely on one man for the coordination of all such activities."[12] But because this necessitated statutory changes, including amending the Budget and Accounting Act, and might involve controversy and delay with "no certainty as to the final outcome," PACGO had a "second best" proposal. This was the appointment in the White House Office of an assistant (or deputy assistant) to the president for management with coordinating and not operating responsibilities. The deputy assistant would aid the president in coordinating the work of the BOB and other units in the EOP. The deputy assistant would report to the assistant to the president (who at the time was Sherman Adams).

Budget Director Brundage was no more sympathetic to PACGO's compromise proposal than to the original recommendation. The budget director viewed the plan as a downgrading of the Bureau's Office of Management and Organization. Brundage registered his official dissent in a memorandum to Sherman Adams, enclosing a copy of Executive Order 8248 which, according to Brundage, "clearly places the responsibility for administrative management squarely in the Bureau of the Budget."[13] Brundage wrote to Adams in a strong defense of the Office of Management and Organization that:

> The provisions of Executive Order 8248 make it clear that the concept of the Executive Office which has governed its organization and procedures for nearly twenty years has been one in which (1) the Budget Bureau has been the President's staff and coordinating agency in the "Management" field and (2) the White House Office and Budget Bureau have been parallel units of the Executive Office performing different duties for the President. *Specifically, staff of the White House Office are not "interposed" between the President and the Budget Bureau.*[14]

PACGO staff director Arthur Kimball believed that "the Budget Director's memorandum proceeds on the basis of several obvious misconceptions as to the

[11] Ibid.

[12] Nelson Rockefeller to President Eisenhower, October 10, 1957. "White House-Executive Organization for Management Activities," *PACGO*, Box 11, File 69.

[13] Percival Brundage to Governor Adams, November 18, 1957. "Executive Office Organization for Management Activities," *PACGO*, Box 11, File 69. Executive Order 8248 transferred the Bureau to the newly established EOP in 1939.

[14] Ibid., pp. 3–4. (Italics added.)

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Committee's intent."[15] Jerry Kieffer and John Kennedy, staff members of PACGO, believed that Brundage's analogy of the Bureau's status in the EOP was an "inconsistency so often practiced by the Budget staff. . . . The Budget Bureau is supposed to be a principal staff arm of the President, [but] really is not a part of his staff but exists separately and apart from him (thus safe from political retribution and restaffing after a change in Administration)."[16]

The Budget Bureau's response to PACGO's proposals is a textbook illustration of Miles' Law—"where you stand depends upon where you sit."[17] From the chair of the BOB director, PACGO's recommendations were viewed as a downgrading of the BOB's Office of Management and Organization and another potential barrier between the budget director and president. From where PACGO was seated the recommendations were seen as restructuring the existing management processes to reduce the president's workload. The Budget Bureau's reaction fits the assertion by Morton Halperin and other writers on bureaucratic politics about how organizations behave when threatened by external change. Halperin notes that "organizations have interests," and that "members of an organization believe that they are in a better position than others to determine what capabilities they should have and how they can best fulfill their mission. They attach very high priority to controlling their own resources."[18] BOB would not yield and therefore the President's Advisory Council was even more steadfastly determined to restructure the Budget Bureau.

The Executive Assistant to the President for Administration. On March 17, 1958, Director Brundage retired and was succeeded by his deputy, Maurice Stans. By April 3, 1958, the Executive Management Act, 1958, had been drafted in the White House. The bill established an executive assistant to the president for administration who would coordinate, supervise, or perform "functions now assigned to the Director of the Bureau of the Budget, the President's Personnel Advisor, and other elements of the White House-Executive Office administrative management organization."[19] The Office of Executive Assistant for Administration would be responsible for developing and coordinating the federal budget; the president's legislative program; staff work in budget, personnel, organization, and management; and planning and program evaluation.

[15] Arthur Kimball and Jerry Kieffer to Nelson Rockefeller, Milton Eisenhower, and Arthur Flemming, November 27, 1957. "Executive Office Organization for Management Activities," Box 11, File 69, p. 1.

[16] Jerry Kieffer and John Kennedy to Dr. Milton Eisenhower, October 1, 1959. "Reorganization of Management Functions in the Executive Office of the President," *PACGO*, Box 12, File 71, p. 2.

[17] This is a well-known dictum of public administration, named for its author, Rufus Miles, long-time HEW and BOB official and former director of the Mid-Career Program in the Woodrow Wilson School at Princeton University.

[18] Halperin, "Why Bureaucrats Play Games," p. 76.

[19] "A Bill to Facilitate the President's Exercise of Certain Executive Functions: The Executive Management Act, 1958," April 3, 1958 (G.D.M.) *PACGO*, Box 11, File 70, pp. 1–2.

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Assistant Director Finan believed that these recommendations "reflected a lack of understanding of the way the Budget Bureau carries out its functions." Finan favored "re-integrating the organizational talent of the Budget Bureau and placing it under a strengthened organization and management unit."[20] PACGO had grown accustomed to Finan's objections and on April 22, 1958, Kimball advised the committee that the "Budget Bureau staff is still strongly opposed to the idea."[21]

The Office of Executive Management. In a meeting with Sherman Adams, Stans agreed that the BOB's management responsibilities needed to be improved and Stans recommended that an Office of Executive Management (OEM) be established by reorganizing the BOB.[22] Director Stans favored any changes that would enlarge and strengthen the BOB without altering the relationship of its director to the president. The budget director's position was based on four propositions:

(1) The Bureau of the Budget could and should be upgraded;
(2) I would oppose the interposition of anyone between the head of the reorganized Bureau and the President;
(3) I didn't have any strong feelings of what the name of the organization should be, but I thought that the word "management" should be in its title;
(4) I was in favor of putting within the upgraded organization the personnel function and other management and organization functions loosely attached to the White House, together with some responsibilities in helping the President with program planning.[23]

PACGO agreed that Stans would be the first director of OEM. The OEM was to have a director, a deputy director, and not more than six statutory assistant directors. The statutory functions of the BOB would be transferred to the Director of OEM. The new agency would have five divisions responsible for budget preparation and administration, legislative clearance and coordination, personnel management, organization and management, and program planning.

Plans for implementing the OEM were initiated in the White House. In spite of Director Stans' support of the plan, Assistant Director Finan was persistent in his opposition to any reorganization of the Bureau's management functions. Finan believed that "a more detailed Presidential explanation as to why [Eisenhower] was abolishing the BOB"[24] was needed. In another textbook example of

[20] Jerry Kieffer, memorandum for the record. "Presidency-Management Functions," December 27, 1957, *PACGO*, Box 11, File 69.

[21] Arthur Kimball to Nelson Rockefeller, Dr. Milton Eisenhower, and Dr. Arthur Flemming, April 22, 1958. "Proposed Executive Assistant to the President," *PACGO*, Box 11, File 70.

[22] Arthur Kimball, memorandum for the record. "Meeting in Governor Adams' Office on May 8, 1958 to consider Proposed Office of Executive Management," *PACGO*, Box 12, File 71.

[23] This information was provided by Mr. Stans in a letter to the author, September 17, 1976.

[24] Kieffer and Kennedy to Milton Eisenhower, October 1, 1958 (see note 15).

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Miles' Law, PACGO staff member Jerry Kieffer wrote to Milton Eisenhower that Finan misunderstood PACGO's intent:

> [Finan's view] stems from the original Budget staff reaction that this whole reorganization is designed to downgrade the BOB. Actually, while the present institution called the BOB would be abolished, the entire range of activities now in the BOB would be enhanced by the greater concentration of functions under the Director of Executive Management. . . . We see no reason for [Eisenhower] to offer a detailed justification with regard to the fate of the BOB.[25]

On November 28, 1958, Kimball wrote to the committee members that the Budget Bureau staff was trying to sabotage the reorganization plan. Observing that "the Budget Boys never give up," and "here we go again," Kimball explained that Finan, who was responsible for drafting the technical aspects of the plan, had "reverted to the earlier Budget staff approach, which the Committee (with Mr. Stans' concurrence) did not favor."[26] Finan had simply renamed the BOB the Office of Executive Management and transferred to it the Office of Personnel Management and other EOP units. Contrary to the Stans-PACGO agreement, moreover, Finan's draft authorized the OEM director and not the president to appoint the statutory assistant directors of OEM.

Meanwhile, Robert Gray, secretary of the cabinet, notified all cabinet members that on the following day, December 12, 1958, President Eisenhower with the assistance of Dr. Flemming would brief the cabinet on "what is intended in the Presidentially-approved reorganization of the Bureau of the Budget."[27] Gray's memorandum explained that an Executive Order would create OEM except for its Budget Bureau elements. The budget director would be named as first director of OEM and simultaneously, a Reorganization Plan would be sent to Congress transferring the BOB to the OEM. Gray requested that the memorandum be handled on an "eyes only" basis and returned to him immediately after the cabinet meeting.

At the cabinet meeting the draft of the reorganization plan was judged to be inadequate and Director Stans was asked to personally draft his plan to the committee members and administration officials. Stans' draft emphasized that BOB was being renamed because "the name BOB obscures the full range of duties of that agency and is an impediment to the achievement of its management role."[28] Kieffer believed that the reorganization should be much more than a renaming of the BOB. A cabinet meeting was called for the next day to discuss the budget director's draft. Kieffer wrote to Gray that in setting the meeting's agenda, sev-

[25] Ibid.

[26] Arthur Kimball to Nelson Rockefeller, Milton Eisenhower, and Arthur Flemming, November 28, 1958. "Executive Office Organization and Management Activities," *PACGO*, Box 12, File 71.

[27] Robert Gray, secretary of the cabinet, to the Honorable Arthur Summerfield, the postmaster general, December 11, 1958. *PACGO*, Box 12, File 71.

[28] See "Reorganization Plan No. 1 of 1959." Draft, January 20, 1959, *PACGO*, Box 12, File 71.

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eral subjects needed "greatest emphasis," because Stans' draft "clearly indicates a Budget Bureau attempt to portray reorganization simply as a renaming of the present BOB."[29]

By January 28, 1959, only eight days after Director Stans had submitted his draft, staff director Kimball wrote Dr. Milton Eisenhower that the BOB was on the verge of defeating PACGO's recommendations because PACGO had been "outmaneuvered by the Budget staff."[30] Kimball's assessment was highly accurate. The Budget Bureau proved itself to be a formidable adversary when threatened with extinction. Kimball did not know, however, that Stans was preparing an internal self-study of the Bureau to serve as an "institutional" response to PACGO's recommendations.

The 1959 Self-Study: A Call from Within for a "New-Style Bureau"

On January 30, 1959, Director Stans informed Budget Bureau staff that an internal self-study of the functions, operations, and workload of the Bureau would be initiated on February 2, 1959. The self-study group could scarcely have been a stronger bastion of veteran BOB staffers. The group consisted of eleven individuals with an aggregate of 246 years of experience in the Bureau (i.e., an average of *22 years each*). Director Stans requested that the group submit their report by May 1, 1959. This necessitated an intensive work schedule, often from 7:00 A.M. to midnight. The conclusions of the study group offer a fascinating example of how two groups (PACGO and the study group) looked at the same problem and because of different perspectives and interests recommended entirely different alternatives.

The study group called upon the Bureau to reorient itself for the challenge of discharging expanded management functions:

> The Bureau must lift its sights from the details of agency operations and develop new devices of management which center upon meaningful, critical factors. While maintaining its staff mission the Bureau must use modern methods focusing on broader principles of management review and control.

The study group recommended that the Bureau adopt a "new style":

> While clinging to basic purposes which are sound, the Bureau must reorient its way of life to keep pace with the new challenges. . . . With such a new style, the Bureau can become a positive influence in helping the President guide the direction of changes of tomorrow. . . .[31]

[29] Jerry Kieffer to the secretary of the cabinet, January 22, 1959. "Office of Executive Management," *PACGO*, Box 12, File 71.

[30] Arthur Kimball to Milton Eisenhower, January 18, 1959. "Personal," *PACGO*, Box 12, File 71.

[31] "Report to the Director: A Self-Survey of the Bureau of the Budget," May 1, 1959, Records of the Bureau of the Budget, *Records Division OMB*, pp. 6-7. The Study group was directed by Wilfred Rommel.

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The 149-page study group report and its sixty recommendations illustrate the Bureau's perception of its future as a reinvigorated BOB but emphatically not a new OEM.

The Executive Assistant to the President and Director of Executive Management

Nevertheless, PACGO (with Rockefeller now departed to be governor of New York) pushed ahead with its proposals. On July 1, 1960, Nelson Rockefeller appeared before the Jackson Subcommittee on National Security Organization and recommended the creation of an executive assistant to the president and a director of the Office of Executive Management "to assist in planning and management in the sphere of domestic affairs."[32] In Rockefeller's view, the president could no longer "undertake to meet the volume of problems and functions today demanding his attention, study, and action." Rockefeller's recommendation represented the culmination of PACGO's efforts to improve the management processes in the EOP. The recommendation was suited perfectly to an Eisenhower-type of president and therein lay its shortcomings, since John F. Kennedy was to be quite different.

Conclusions

The Rockefeller recommendations were adopted neither by Congress nor by the next administration. David Bell, President Kennedy's first budget director and a former Bureau employee during the "golden days" of the BOB under James Webb (1946–1949), believed that the Bureau had not been used to its fullest capabilities during the Eisenhower years. Bell left the agency's reorganization intact and molded the Bureau's Office of Legislative Reference into an appendage of the White House staff in the policy-making process.

The Bureau had won a reprieve against an advisory group recommending that the administrative management processes in the EOP could benefit from the reorganization of the Budget Bureau. The Bureau's initial victory was only a portent of future onslaughts by management-oriented presidential advisory groups. In 1967 and 1970, two presidentially appointed advisory groups arrived at the same conclusion and the Budget Bureau was once again fighting for its survival.

The Heineman Task Force on the Organization and Management of Great Society Programs

In 1966 a little-known and previously unavailable study of the organization and management of Great Society programs was undertaken by a Johnson admin-

[32] Hearings before the Subcommittee on National Policy Machinery of the Committee on Government Operations. U. S. Senate, 86th Congress, 2d sess., June 28 and July 1, 1960, part VII, p. 947.

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istration task force. Since that time, a few writers have referred to the study but a comprehensive analysis of its recommendations is unavailable. In 1969 William Carey, a former senior BOB official, reported that Johnson buried the report "in the presidential desk drawer and applied a total embargo on both the distribution of copies and any mention of the fact that there existed such a task force."[33]

The Heineman task force reports have now been released by archivists in the Lyndon Baines Johnson Library. The task force's blue ribbon membership included McGeorge Bundy, William Capron, Hale Champion, Kermit Gordon, Herbert Kaufman, Richard Lee, Bayless Manning, Robert McNamara, Harry Ramson, and Charles Schultze. The group submitted four reports to President Johnson: (1) *The Organization and Management of Great Society Programs;* (2) *Government Organization for Economic Policy Formulation and Administration;* (3) *A Recommendation for the Future Organization of the Executive Branch;* and (4) *Organization for the Management and Coordination of Foreign Affairs.* This article will focus exclusively on the task force report on *The Organization and Management of Great Society Programs,* which identified several shortcomings in the president's staff facilities in intergovernmental management and program formulation.

The Management Problem

Intergovernmental Management. The Heineman task force rejected the political criticism of those who opposed the goals of the Great Society, but they agreed that the "organizational criticism was merited."[34] The hundreds of legislative programs developed from 1963–1966 had "put a great strain upon obsolescent machinery and administrative practices at all levels of government."[35]

The social programs of the 1960s were indeed radically different from previous federal programs. The target problems were no longer single, one-of-a-kind problems. The Heineman report observed that:

> In facing the full dimensions of domestic problems the President has challenged the simple faith in a series of narrow, unrelated programs conceived by and managed from Washington. It is no longer possible to believe that the problems of the poor can be solved by a series of isolated efforts—whether in housing, or health care, or job training. Organizationally the goals of Great Society legislation demand collaboration and cooperation—between Federal bureaus and departments AND between governments.[36]

[33] William Carey, "Presidential Staffing in the Sixties and Seventies," *Public Administration Review,* 29 (September/October 1969), 452.

[34] The papers of Fred Bohen, staff director of the Heineman task force, James Gaither, and Joe Califano contain the official report and several working papers. Cited hereafter as *Heineman Report.*

[35] *Heineman Report.** (A final draft of the report dated June 15, 1967, was condensed for the president's reading. The asterisk refers to the unabridged draft of the report.)

[36] *Heineman Report,* p. 6.

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The president's management job was viewed as the "largest and toughest in the world" because, unlike other executives, the president could not give management his complete attention. The task force recognized that "a permanent dilemma for Presidents is that the Presidency has too many dimensions for one man to handle alone yet also is constitutionally indivisible." This raised a managerial challenge to the president:

> The managerial challenge to the President, therefore, is to keep a firm hand on the domestic tiller by reserving big decisions to himself and by delegating the next level of his work to intimate staff lieutenants who command his full confidence and share his hopes and plans.[37]

Program Leadership. Nowhere in the EOP did the president have an institutionalized staff for comprehensive program planning and consequently, "a portion of our present problem of organization and coordination has been caused by the way new social programs were developed and enacted."[38] The Heineman group identified five major drawbacks in the existing practice of planning new programs:

—It relies heavily on chance.
—It focuses almost entirely on shaping specialized programs, neglecting the vital job of creating a coherent overall Federal program strategy.
—It tends to neglect emergency or complex problems for which no agency has particular responsibility.
—It is only loosely related to careful study of social problems and review of existing programs.
—It provides no regular mechanism for deciding between expansion or modification of existing programs and creation of new ones.[39]

It is important in understanding the Heineman recommendations to recognize that the task force viewed management and program leadership as two parts of the same organizational problem. The president needed improved mechanisms for managing intergovernmental programs *and* improved program planning resources for formulating those same programs.

The Heineman Recommendations

The Office of Program Coordination (OPC): The Intergovernmental Management Solution. The task force recommended that an Office of Program Coordination be established as a new, independent staff unit in the EOP with a field force organized in ten federal regions. Program coordination was identified as "the greatest unmet need" of the president's management responsibilities. The Heineman report explained that "the job of program coordination was much too large to be done by a few talented people on a part-time basis," which had been stan-

[37] *Heineman Report,** p. 1.
[38] *Heineman Report*, p. 10.
[39] Ibid.

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dard operating procedure. The time had come "to institutionalize these responsibilities and provide staff to discharge them comprehensively and effectively."[40]

The OPC director and his staff would be expected to assume responsibility for several important management functions in the intergovernmental area:

—Anticipate, surface, and settle jurisdictional and program arguments between Federal departments. . . .
—Monitor the administration of Great Society programs requiring cooperation between Federal departments.
—Provide the staff focal point in the White House, short of the President himself, for continuing consultation with Governors and Mayors in domestic programs and problems of intergovernmental cooperation.
– Carry primary staff responsibility in the Executive Office for longer term efforts to improve intergovernmental organization relationships, through grant consolidation, harmonization of financial matching arrangements, etc.[41]

The Heineman group had well-articulated reasons for locating the OPC outside of the Budget Bureau. Domestic program coordination, they argued, is "a large job with short-term operational as well as long-term intergovernmental systems development dimensions. We doubt that it would get the necessary emphasis if incorporated with BOB's two main functions—program evaluation (budget) and program development."[42] The OPC's success depended upon its ability to influence line agencies in the implementation of federal programs. It was a day-to-day and immediate problem of program coordination, far too demanding to break into the BOB's already highly rigorous, formalized budgetary and legislative clearance cycle with its requirement that legally mandated deadlines be met.

The Office of Program Development (OPD): The Program Leadership Solution. The president needed not only a management control instrument, but also an institutionalized staff for problem definition, program design, and program coordination. Four years of task forcing, ad hoc problem solving, and underfunded social programs had undermined the quality of legislative performance. The president needs "smart men to design his programs just as much as he needs strong men to implement them. He needs brainpower loyal to him, and in touch with his broad perspective."[43] The task force recommended, therefore, that an Office of Program Development be established within a reoriented Budget Bureau and headed by a presidential appointee of executive rank.

The OPD, if initiated, would assist the White House staff in the program-development process. The new office would:

40 Ibid., p. 9.
41 Ibid., p. 8.
42 *Heineman Report,** p. 18.
43 Ibid.

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—Analyze complex or emerging problems.
—Keep in close touch with the nation's best research efforts—outside as well as inside the government—relevant to social problems and policy.
—Design, review, and propose modifications in government programs, aimed at domestic problems.
—Stimulate better analysis and program development in the executive departments.
—Stimulate the establishment of, and frequently staff, Presidential Commissions and task forces.
—Relate individual programs to a general administration strategy and evolving priorities.[44]

The OPD was to be located within the BOB on the assumption that "a substantial reorganization of the Bureau of the Budget be undertaken with a view to widening its responsibilities and revising its priorities." The Heineman report emphasized that "Reorganization should speed up sharply the Bureau's transition from intelligent reaction to departmental demands to active, independent leadership in program development that supports and is responsive to the President and his perspective."[45]

Chairman Heineman favored renaming the Bureau of the Budget to the Bureau of Program Development and Management in order to "dramatize the magnitude of the change in role." A staff paper prepared by Allen Schick recommended that program planning become the central responsibility of the Budget Bureau. Schick argued that the Bureau would have to "reexamine its mission and purposes and its role as a Presidential staff" and that the Bureau "must become the center for policy studies in the EOP."[46]

The Heineman task force believed that their recommendations would not "solve" the president's management problem. Instead, they were intended to be "tools" with which the president could enforce overall coordination of federal programs. The recommendations "should be seen not as final answers but as steps to strengthen Presidential managerial authority and leadership where it now is most needed."[47]

The final report of the President's Task Force on Government Organization was submitted on June 15, 1967. President Johnson refused to take any action on the recommendations and ordered his staff to keep the document secret. During the 1968 presidential transition, Johnson's transition coordinator, Charles Murphy, received a request from president-elect Nixon's representative, Frank Lincoln, for a copy of the Heineman report. Murphy wrote Johnson that because "Joe Califano's feeling is that you should not make the report available to the President-elect but should keep it for your own use," should Murphy give the

[44] *Heineman Report*, pp. 11–12.
[45] Ibid., p. 10.
[46] Allen Schick, "Some Budgetary Prerequisites for Program Planning: A Critical Analysis," p. 19. *Bohen papers*, Johnson Library.
[47] *Heineman Report*,* p. 25.

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Heineman report to Nixon? Johnson's answer, scrawled across the top of Murphy's memo, was "Hell, NO."[48]

The Budget Bureau's Official "Institutional" Response: The 1967 Self-Study

Johnson's refusal to act on the Heineman recommendations presented the Budget Bureau with an opportunity to seek to fend off further outside reorganization efforts by upgrading its management functions. The vehicle for this effort was the recommendations of a 1967 steering group evaluation of the BOB which Director Schultze had commissioned as early as February 1967.

On February 24, 1967, Budget Director Charles Schultze informed all office and division chiefs in the Budget Bureau that an evaluation of the Bureau's organization and management would be undertaken immediately. Director Schultze cited several reasons for the evaluation: (1) the passage of seven years since the last evaluation—A Self-Survey of the Bureau of the Budget, May 1, 1959 (another time the BOB was under scrutiny by an advisory group); (2) the proliferation since 1960 of ad hoc policy framing arrangements—task forces, for example, which tend to overlap Bureau/Executive Office responsibilities; and (3) the multiplicity of program, funding, and management problems growing out of passage of Great Society legislation in the past two years. Schultze explained that the self-study was essential because "only through such a review can we be sure that we are discharging our responsibilities to the President and to the departments and agencies effectively and efficiently."[49]

Director Schultze, a member of the Heineman task force, initiated the self-study at a time when the Heineman group was first shaping its recommendations. A staff member of the Heineman task force explained that "Schultze saw where the Heineman group was moving in its critique of the Bureau and he decided to implement his own study to see what was wrong." Schultze was not out to undermine the recommendations of the presidential evaluation group. He believed, instead, that if President Johnson did not implement the Heineman recommendations the budget director would make the necessary changes. Consequently, when President Johnson shelved the Heineman proposals, Schultze seized the opportunity to reorganize the Bureau in accord with the self-study recommendations.

The final report of the steering group (July 1967) acknowledged that "the interagency-intergovernmental nature of much of the recent social and economic legislation has brought with it unprecedented problems in the organization and management of the executive branch. This in turn has required the Bureau to re-

[48] See Charles Murphy to President Johnson, November 22, 1968. "Heineman Task Force Report," FG 11-8 1969, Johnson Library.

[49] Charles Schultze to Office and Division Chiefs, February 24, 1967. "Evaluation of the Organization and Management of the Bureau of the Budget," *1967 Self-Study*, OMB Library.

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evaluate its Government-wide responsibilities in the area of organization and management."[50]

The Bureau's Office of Management and Organization, the same office with which PACGO had battled, was identified as a "depressed area in the Bureau."[51] The steering group recommended that OMO be abolished and replaced by an Office of Executive Management under the direction of a statutory assistant director. The OEM was expected to focus on interagency-intergovernmental management programs. The steering group's recommendations are interesting when compared with the Heineman recommendations because the OEM resembled Heineman's OPC but located within the BOB, rather than being a new EOP unit.

Conclusions

The Budget Bureau's 1967 self-study illustrates BOB's institutional reflex when it came under scrutiny by an outside, presidentially appointed advisory group. In 1967 the Bureau found itself in the position of having the Heineman recommendations ignored by the president, and consequently the agency initiated its own reorganization. The Bureau reorganization had the advantage of legitimacy, but it also meant that if performance did not improve the Bureau would have no one but itself to blame.

The Heineman report provides an important link between the PACGO and Ash Council recommendations insofar as the BOB was concerned. The task force was initiated by a Democratic administration and sandwiched between two Republican administration advisory groups. Yet, all three groups agreed that the BOB could not adequately discharge its management responsibilities.

In 1969 Roy Ash cited *both* Eisenhower's January 1961 budget message calling for the establishment of an OEM and the Heineman critique of the organization and management of Great Society programs as precedents for the Office of Executive Management (later changed to OMB).

The Ash Council: Final Achievement of Structural Reform

During the 1968 presidential transition, president-elect Nixon established a fifteen-person transition task force chaired by Frank Lindsay, president of ITEK Corporation of Lexington, Massachusetts. The Lindsay Task Force recommended that "the President-elect give first priority to organizing more effectively the White House-Executive Office as the best way to improve the operations of the

[50] "The Work of the Steering Group on Evaluation of the Bureau of the Budget," July 1967. OMB Library, pp. 2–15.

[51] "Organization and Management Activities of the Bureau of the Budget," May 2, 1967, *1967 Self-Study*, OMB Library, p. 3.

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entire Executive Branch."[52] Nixon was advised to renew the recently expired Reorganization Act and to establish an advisory committee to study the organization of the executive branch. On March 27, 1969, Congress renewed the president's reorganization authority and on April 5, 1969, Nixon established the Advisory Council on Executive Organization chaired by Roy Ash of Litton Industries. The six-man council included John Connally, Richard Paget, Fred Kappel, George Baker, and Walter Thayer.[53]

The Management Problem

In an August 20, 1969, memorandum to President Nixon, Roy Ash explained that the President's Advisory Council had "analyzed studies made since 1939 on Presidential management and conferred with many contributors to those studies. Regardless of ideology or party, there is virtual unanimity that organizational improvement of the Executive Office of the President is needed."[54]

The council's staff initially compiled a list of 100 organizational problems in the executive branch and later reduced this to a more manageable number of 10 problems, which needed immediate solving. One of these problems was executive office organization. The Council then isolated six major management processes in the executive office which, because of organizational inadequacies, needed bolstering. The council's list was remarkably similar to the problems identified in the Heineman report:

1. Development of policies and programs; that is, the translation of national objectives into specific, workable, and consistent actions or recommendations to the Congress.
2. Attention to the organizational and management implications of program decisions—so that the means for implementing programs receives the same emphasis as their formulation, their legislation and their appropriations.
3. Evaluation of programs—assessing program achievements in terms of performance objectives and cost.
4. Creation of an information system—to systematically and continually inform the President and the agencies about the many things that they must know.
5. Resolution of interagency problems of coordination as they arise in the field; that is, when the operating agencies involved cannot reach agreements on a solution that best serves the total public interest.
6. Development of executive career personnel.[55]

[52] The *Lindsay Task Force Report*, cited in the Ash council report to President Nixon, August 20, 1969.

[53] There is almost nothing in the published record concerning the work of the Ash council. The official report has not yet been released but I have had access to individually owned copies of the report and will therefore be able to draw on the report in the analysis that follows.

[54] Roy Ash to President Nixon, August 20, 1969. "The Executive Office of the President," p. 1; cited hereafter as *Ash memo No. 1*.

[55] *Reorganization Plan No. 2 of 1970* (Office of Management and Budget; Domestic Council),

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In an October 17, 1969, memorandum to the president, Ash explained that "in recent years, billions have been invested in domestic programs, yet adequate means for the President to manage them have neither been requested nor provided."[56] The Ash council addressed itself to recommending the institutional machinery that would enable the president to manage the executive branch.

The Ash Recommendations

Ash's 1968 Transition Recommendations. When Roy Ash was selected to head the president's Advisory Council, Nixon was already familiar with Ash's ideas on executive office organization. During the 1968 transition period Ash had prepared a ten-page memorandum for the president-elect on "Executive Office Organization," which argued for an Office of Executive Management (OEM). Ash's OEM would encompass the BOB's budgetary responsibilities and add three new functions: a Division of Evaluation, a Domestic Policy Council (DPC), and a Division of Program Coordination. On the basis of this ten-page memo and diagram, Nixon instructed Ash to prepare his reorganization plan for implementation.

Nevertheless, the Ash plan was dropped when the incoming administration, facing an opposition Congress, learned that congressional approval would be necessary to implement the plan. President Nixon's first budget director, Robert Mayo, was never even told of the plans for OEM because it was feared that the budget director would utilize the resources of the BOB against the plan if he realized that his organization might one day be subsumed by an OEM. Mayo "accepted his post without full understanding that a higher official—the Director of OEM—might one day be placed over him."[57] Consequently, Ash's recommendations were set aside until the Ash council's first proposals were sent to President Nixon on August 20, 1969.

The Ash Council's First Recommendations for an OEM and DPC. For several months the staff of the council reviewed all previous reform proposals for the executive office, interviewed over 200 people including two former budget directors, several former and present cabinet officers, and former President Johnson. On August 20, 1969, the council forwarded its first recommendations to President Nixon. The council agreed with the problems identified in the Heineman and Lindsay reports and recommended that the president establish (1) an Office of

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Hearings before a Subcommittee of the Committee on Government Operations, House of Representatives, 91st Congress, 2d sess., April 28, 30, and May 5, 1970, p. 8.

[56] Roy Ash to President Nixon, October 17, 1969. "Proposed Organization of the Executive Office of the President," p. 3. Cited hereafter as *Ash memo No. 2.*

[57] Attributed to Richard Tanner Johnson, staff member of the Ash council. Nixon asked Ash to be his first budget director, but Ash declined the offer. This was confirmed by Roy Ash in a conversation with the author.

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Executive Management to assist the president in managing the federal government and (2) a Domestic Policy Council to provide "a place where major domestic programs may be evaluated against each other and against available resources, and integrated for maximum effectiveness."[58]

There was a major difference between Ash's transition recommendations to the president and his proposals of August 20. In the transition memo, the DPC was to be located within a larger OEM. To obtain John Ehrlichman's support for the reorganization plan, however, "the DPC which had originally been a functional division of the OEM, was moved outside of that structure and given an independent reporting relationship to Ehrlichman and the White House."[59]

The Ash Council's Second Recommendations for an OEM and DPC. President Nixon was interested in the August 20 material, but he requested that the council be more specific in their recommendations. Two months later, on October 17, the council forwarded a revised set of recommendations to the president. The council's selling point was that the OEM and DPC represented "the first major improvement in the President's management capability since the Brownlow Commission recommendations were implemented by President Roosevelt in 1939."[60] The council's recommendations were based on their concern "that the increased pace, scope, and complexity of national affairs requires improvement in the managerial capacity of the President's office."[61]

The Domestic Policy Council, later changed to the Domestic Council, was described as an office where the total range of domestic policy could be coordinated and developed. The DPC would make forecasts, synthesize policy alternatives, provide rapid response for presidential emergencies, and constantly revise ongoing programs when necessary. The DPC would be the key link between the president and the agencies, communicating the president's goals and philosophy to the agencies and providing the president with agency positions.

The Office of Executive Management was envisioned as the president's chief management arm, providing "enhanced institutional capability for budget and program evaluation, program coordination, legislative clearance, executive personnel development, and organization and management systems improvement."[62] The report emphasized that OEM was not simply the renaming of BOB, but rather, that OEM was a "fundamental departure and innovation." It would be result-oriented and "embody a concept of management that goes well beyond the present emphasis on budget. . . . A substantially different orientation to the President's managerial job will emerge."[63]

[58] *Ash memo No. 1.*

[59] Attributed to Richard Tanner Johnson and verified by Roy Ash.

[60] *Ash memo No. 2*, p. 3.

[61] Ibid.

[62] *Ash memo No. 2*, p. 8.

[63] Ibid., p. 20. The OEM would have five divisions: Budget and Evaluation, Program Coordination, Legislative Clearance, Executive Personnel, and Organization and Management Systems.

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President Nixon agreed with the October recommendations and requested the council to prepare the necessary legislation. The Ash council expected to encounter some congressional and BOB opposition, but certainly not enough to defeat their plan. This proved to be the council's major tactical error because BOB's opposition to the Ash recommendations was strikingly similar to the agency's reaction to PACGO's reorganization proposals.

The Bureau's Last Stand

The top career staff of the Budget Bureau openly opposed the Ash council recommendations for an OEM. According to Ash:

> One of the biggest problems in trying to convert the Bureau of the Budget to the Office of Management and Budget was that the pocket of greatest resistance, of political resistance, was the Bureau of the Budget. Some of those guys went up to the Hill to sabotage that thing, and to really turn the screws up on some of them. That was the biggest resistance block right there.[64]

Budget Director Mayo opposed the plan and tried to convince Ash and Ehrlichman that the reorganization was unnecessary, because in the past year the BOB had increased the size of its management staff under the leadership of Dwight Ink. The budget director's staff asked Mayo if they could publicly fight the Ash recommendations but Mayo refused to do so. He told them that he would continue to oppose the recommendations inside the White House but he did not want a public confrontation, which would pit the BOB against the president and Ash.[65]

Mayo's reservations included the omission of the OEM director from the DPC; the Ash council's belief that budget making could be separated from policy making; that DPC staff would be permitted to participate in the budget director's internal review of agency budget requests; and that Congress would object to the delegation of all BOB functions to the president who would then delegate them to the director of OEM. Most of these were watchdog functions which belonged with the director of OEM, not the president.

The Ash council and several White House staff aides realized that the Budget Bureau was trying to sabotage the recommendations. In a conversation with Mayo, Ash "observed that BOB has many ways at its disposal to kill the plan and asked Mayo's assurance that neither he nor his people would sabotage the plan through extra-legal channels."[66] The Bureau did not revert to a self-study as in 1959 and 1967. Rather, it relied on more subtle defense procedures to offset the Ash proposals. First was a Bureau announcement that James Frey would be appointed to fill the long vacant post of executive assistant director. The executive assistant was responsible for directing the internal management process of

[64] Interview with Roy Ash, November 6, 1975.
[65] Interview with Robert Mayo, October 13, 1976.
[66] Attributed to Richard Tanner Johnson and verified by Robert Mayo.

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the BOB. The bulletin stated that "Frey's new role will emphasize the continuing institutional function of BOB as the principle career staff for the President." Following the Frey appointment, Andrew Rouse of the Ash council observed that "the Bureau had filled this post several times in its fifty-year history—each time when it was under attack."[67]

The second example of the Bureau's defense mechanism centered around the Ash council's request for an additional million dollar budget request for FY71. Johnson recalled that:

> The BOB had very little desire to see the Council continue. They had simply "misplaced" the Council's budget request until it was too late to incorporate it into the President's Special Fund Budget. This had gone to the printers a week earlier. The BOB was thus forcing the Council to make a special line-item request to the Congressional Committee. Such a request was vulnerable to Congressional veto.[68]

The Bureau's defense tactics shifted into high gear when Reorganization Plan No. 2 of 1970 was sent to the House Subcommittee of the Committee on Government Operations. On May 5, the Subcommittee reported out a resolution of disapproval on Reorganization Plan No. 2 leaving the Ash council "shocked" by this decision. Johnson recalled that one Council member remarked: "You've got to hand it to the BOB . . . they snookered us into complacency until it was too late."

The Ash Council Counterattack

With the Subcommittee's recommendation of disapproval the odds were not favorable that the reorganization plan would be approved by a vote of the full House. The council initiated a massive lobbying effort on the Hill to gain passage for their recommendations, but the staff seriously doubted that they would succeed. For example, Golde located in the PACGO files Kimball's 1959 memorandum to Rockefeller noting "Here we go again; the Budget boys never give up" and passed it among the council members. The council staff started "speculating upon whether the BOB will be as successful as they were in 1959 in blocking the reorganization proposals."[69]

The council ultimately overcame the Bureau's opposition by playing to the Budget Bureau's traditional weaknesses. The Bureau could never win a popularity contest with Congress and the Council portrayed the Bureau as an unresponsive policy-making institution which needed to be curbed.

On May 13, 1970, the House voted 164 for and 193 against the disapproval resolution. On July 1, 1970, Reorganization Plan No. 2 of 1970 became effective, the Office of Management and Budget (OMB) replaced the BOB, and a Domestic

[67] Attributed to Richard Tanner Johnson.
[68] Attributed to Richard Tanner Johnson.
[69] Attributed to Richard Tanner Johnson.

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Council was established in the White House.[70] The Ash council succeeded because the Bureau's popularity with Congress was at an all time low and many congressmen believed that institutional changes were required. The reorganization marked the victory of presidential "intent" to improve the management processes of the EOP through reorganization of the Budget Bureau. The Bureau had been remarkably successful from 1957–1969 in thwarting any recommendations from "outsiders" for internal BOB change. Perhaps the most appropriate eulogy was delivered on the eve of reorganization by William Carey. "The Bureau of the Budget had a good thing going until the cheering stopped,"[71] he observed. No one knew this better than the BOB staff, PACGO, the Heineman task force, and the Ash council.

Conclusion: Does the OMB Make Any Difference?

From the BOB's point of view, the Ash council recommendations were seen as an explicit downgrading of the Budget Bureau. From the Ash council's perspective, the recommendations represented an upgrading of the agency's management functions by downgrading its budget responsibilities. This statement is more than double-talk. It goes to the heart of how institutions and the career staff who inhabit them are prone to behave when threatened by "outside" reform proposals.

Bureau veterans, always proud of their agency's status at the center of the resource allocation system in American politics and the agency, developed internal norms to reflect this attitude. The Bureau was "the place to be" and most BOB old-timers will tell you that the new OMB cannot match the expertise and staff work of the old BOB. These sentiments were best expressed by a former senior official of the Bureau: "We were, in the Bureau, under Democrat and Republican administrations a kind of working family. . . . It was a growing experience. . . . No one was trained to be a Bureau of the Budget man in a formal sense. You learned by following and by watching. . . . People would take you aside and tell you what they knew. It was sort of like an aging quarterback trying to teach someone coming up as a rookie. There was a real quality in the place. . . . All that's gone now."

The Bureau fought the PACGO and Ash recommendations (and "reflexed" to the Heineman proposals) when these internal norms were threatened. Although there is no doubt that the real issue for the BOB should have been increasing its power or effectiveness as either an OEM or OMB, it was the style

[70] The name OEM was replaced at a late stage in the council's preparation of the president's reorganization message. At first OEM was changed to the Office of Management, but many congressional leaders opposed the dropping of "Budget." Two days before the plan was submitted to Congress, the name was changed to OMB.

[71] William Carey, "Reorganization Plan No. 2," *Public Administration Review*, 30 (November–December 1970), 634.

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and norms of the BOB that were challenged in 1959, 1967, and 1970. PACGO, the Heineman task force, and the Ash council did not invent the problems that needed remedy. In 1959 and 1967 Bureau-initiated self-studies uncovered the same serious problems in the agency's organization, mission, and staffing. The Bureau's solution, however, called for a "new style" Bureau and not a "new" OEM or OMB.

Reorganization Plan No. 2 of 1970 provided the president with an increased institutional staff capability in program evaluation and coordination, government organization, information and management systems, and in the development of executive talent. Nevertheless, did these changes and the final victory of presidential "intent" produce significant improvement in OMB's management role?

In October 1971, OMB sponsored a Federal Management Improvement Conference which was attended by over 300 managers from 34 departments and agencies in the federal government. It was clear to these federal managers that OMB had made only marginal gains in the management area since Reorganization Plan No. 2 of 1970 was implemented. Of the conferees who attended panels on OMB, 59 percent observed no significant changes in OMB compared to BOB and only 35 percent believed that OMB gave more emphasis to management issues than did BOB.[72] Only 17 percent of the conferees believed that OMB was more effective than its predecessor and 65 percent recommended strengthening the management functions of OMB.

A subpanel entitled the "Impact of the New Management Emphasis on the Program Divisions and the Budget Process" arrived at several disturbing conclusions regarding the effectiveness of management improvement efforts since 1970:

1. There has been little discernible impact on the budget examining process as a result of the OMB's new emphasis on management.
2. There appears to be little change in the Examiner's approach to reviewing program budgets.
3. Examiners appear confused and uneasy about improving management of the Federal apparatus through the budget process in which they participate. They display little understanding of what these management objectives may be.
4. There appears to be no systematic communication between the budget and management functions in OMB on matters of mutual concern.
5. The OMB has not defined for the agencies what it means by "Management," nor what its specific objectives may be.[73]

In September 1976, this author administered a questionnaire to all professional and nonprofessional employees in OMB,[74] in part to ascertain OMB personnel's

[72] "The Office of Management and Budget's New Role—An External View," Panel No. 3, *Federal Management Improvement Conference*, October 19, 1971, p. 2. *OMB Records*.

[73] Ibid., Sub-Panel 3-A, pp. 8–9.

[74] Larry Berman, "Study of the Office of Management and Budget." The questionnaire and

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perceptions of management improvement efforts in their agency since 1970 and the adequacy of OMB as a presidential management staff. Well over 50 percent of the respondents gave OMB a "fair" to "poor" rating when asked to evaluate the agency's management performance (compared to a 12 percent negative response when evaluating budgeting performance). When ranking the actual emphasis accorded budgeting and management in the day-to-day work of OMB, 86 percent of the respondents gave budgeting a "High Priority" compared to 14 percent in answer to a similar question for management. One OMB professional wrote that "the management side suffers from a lack of direction and sense of purpose. Its structure is fragmented with several units involved in the same or similar functions." Another respondent asked "Is OMB really the place to have management responsibilities?" Management concerns are still the first trade-off in OMB and the agency's implementation and evaluation efforts are almost nonexistent. The management divisions are involved in "Special Projects" work and, as one respondent wrote, "Management divisions are perceived as having very little to do in terms of legislation, budgeting and management." It appears that little has changed since a working paper prepared for the 1967 steering group evaluation of the Bureau observed that the "everyone knows about management" phenomenon helped reinforce "the low priority placed by the Bureau on organization and management competence."[75] *

data are part of a doctoral dissertation entitled "The Evolution of a Presidential Staff Agency: Variations in How the Bureau of the Budget-Office of Management and Budget Has Responded to Presidential Needs," Princeton Univesrity, June 1977. A second mailing was conducted in February 1977.

[75] "Organization and Management Activities of the Bureau of the Budget," May 2, 1967, p. 25. *OMB Library.*

* This is a revision of a paper delivered at the annual meeting of the American Political Science Association, Chicago, 1976. I am especially indebted to Professor Fred I. Greenstein for commenting on earlier drafts of this paper. Roy Ash, Fred Bohen, Murray Camarow, Ben Heineman, Roger Jones, Robert Mayo, Harold Seidman, Herman Somers, and Maurice Stans commented on all or parts of the manuscript. Melvin Margerum of OMB Records and Elizabeth Knauff and her staff at the OMB Library provided invaluable assistance.

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Book Reviews

America as an Ordinary Country: U. S. Foreign Policy and the Future *edited by Richard Rosecrance. Ithaca, N. Y., Cornell University Press, 1976.—276 pp. $9.75.*

"Once so dominant in the international arena, America has become an ordinary country in foreign relations. . . . The *Pax Americana* is over . . . she cannot be expected to take on special responsibilities for world peacekeeping, to use her military forces where others do not wish to become involved, to support the world economy and sustain an international financial structure organized around the dollar. Her role as a maintainer of the system is at an end" (p. 12). It is to analyze this more limited role in the post-Vietnam era that the editor has collected a group of distinguished authors.

Alastair Buchan, taking a broad perspective, is his usual suave self. Coral Bell, in what is in many ways the most interesting article, evaluates détente and finds that, on balance, it has greatly benefited the United States; Pierre Hassner analyzes the contradictions of United States policy toward Europe and François Duchêne examines whether in the future this policy should be directed more toward the European Community, despite the current pessimism about its viability; in case it is not, Peter Katzenstein speculates about a special West German-United States relationship; Kenneth Hunt turns to American policy in Asia; Leonard Silk, Fred Bergsten, and Robert Gilpin examine the newer economic side of United States policy, especially the changing American multinational corporations' relationships toward their host and home countries; and Richard Rosecrance concludes with two essays, one critical of past policy and the other prescribing for the future.

Interesting as the book is, it unfortunately comes near the end of a long line of books reexamining America's role after Vietnam and OPEC. Further, despite the editor's repeated emphasis that the United States, given Europe's political paralysis, can use its Chinese connection to check Soviet ambitions, there are three lengthy essays on American-European relations and little on the ability to exploit the contra-

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dictions of Sino-Soviet and Warsaw Pact relationships. Moreover, while three articles stress economic affairs, the focus is so much on the MNC that the problems of resources, including oil, population, and food, as well as monetary and ecological problems suffer by comparison; there is little analysis of the increasing First–Third World clashes which will surely occupy much of the time of future American policy makers, whether that relationship will continue to be one of confrontation or, hopefully, become one of greater accommodation in a multilateral effort to build a world order in which the underdeveloped countries will have a greater stake.

Why the United States, however, is called an "ordinary" country is puzzling. Is it because of its relative decrease of power? Yet, how much has that power shrunk, given our immense agricultural production, technological innovation, and continuing economic strength which may have *enhanced* our power against Russia and confers great strength for future negotiations with the Third World? Or is it a decline of American will and a definition of a role that allegedly will achieve more for less effort? And while a more flexible United States role is certainly required, is not this book and all the problems it examines or touches upon which this country is supposed to take a lead in solving, a repudiation of the title? "Ordinary" or "extraordinary"? Indeed, are these essays not testimony to America being the one *indispensable* power in the world without whom virtually none of its problems can be resolved?

JOHN SPANIER
University of Florida

Developing the ICBM: A Study in Bureaucratic Politics *by Edmund Beard. New York, Columbia University Press, 1976.—xiv, 273 pp. $15.00.*

Edmund Beard has written a thorough and well-documented account of the technical, organizational, and strategic problems that hindered the initiation of a viable American ballistic missile program after World War II. Two aspects of the study in particular suggest that it is likely to become required reading for all those with even a general interest in the fields of weapons development or missile technology.

First, Beard goes far toward dispelling some widely held notions about why the United States government was so slow in developing the ICBM. The standard explanation has been that the weight of first and second generation warheads was so great that they would have required huge and inaccurate delivery vehicles. Only when these weight problems were supposedly resolved in 1953–1954 did a reasonably sized, accurate ICBM become feasible. The author, however, marshals convincing evidence to support his contention that the problems of warhead weight and effectiveness had become manageable as early as 1948–1949. The lack of progress between World War II and 1954 was primarily a function of organizational rigidity and bureaucratic struggles within the military establishment, rather than technological constraints.

Second, by focusing on a set of program policy decisions that stretched over a ten-year period, the study succeeds in demonstrating the recurrent and important—

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but often undramatic—impact of bureaucratic factors on political outcomes. Beard leads the reader through the labyrinth of the complex military structure that was evolving in the immediate post-World War II political environment, stopping at appropriate places to point out exactly how structural reorganizations, intraservice rivalries (such as that between the Air Research and Development Command and its parent Air Materiel Command), and interservice squabbles affected the fate of the ICBM program. Particularly convincing is his argument that one of the central reasons why ballistic missile development got off to such a weak start was the predisposition among Air Force personnel to avoid what they perceived as a high-cost, high-risk venture. The inability of planners to break out of this cautious bureaucratic mind-set, and to recognize a viable alternative to the manned bomber, led to a lack of commitment and funding. The result, argues Beard, was a self-fulfilling prophecy that rapid progress toward a functioning ICBM system was highly unlikely; and therefore that an increase in organizational support was unwarranted.

Despite its general strengths, however, the study is not without its problems. By restricting his focus primarily to the military bureaucracy, the author is able to provide us with an understanding of only a small part of the total political process. Had he been able to escape the narrow confines of the bureaucratic politics perspective and adopt a more inclusive model, readers would be better able to place the role of military agencies in the larger political context within which they operated. A series of crucial decisions involving actors outside the bureaucracy—President Eisenhower, Congress, the Millikan Committee, RAND, defense contractors, and the von Neumann Committees—which did finally boost ICBM development into a high-priority program, receive only scant attention. A more wide-ranging investigation might well have found that the bureaucracy remains a relatively weak component in the political process, exercising freedom of action only within the parameters established by other actors.

Another basic problem is the author's tendency to intertwine evaluation and prescription in with his explanations of bureaucratic behavior and its consequences. Beard wants not only to show that bureaucratic factors impeded the development of the ICBM, but also to demonstrate that they should not have been allowed to interfere with the R&D process, and to suggest ways that such "problems" can be avoided in the future. For example, the author declares that "a serious research and development program should have been directed at the ICBM in order to inspect the technical issues and attempt to prevent any unfortunate surprises. This was never adequately undertaken until 1954" (p. 9). Such blending of normative and analytical judgments raises the possibility of implicit bias in what is supposed to be an "objective" account. Given the uneven quality of the information sources, the author is frequently forced to impute motives in order to explain the behavior of individuals and agencies. We can only hope that he did not permit his preconceptions about what should have happened to taint an apparently sound analysis of an important and complex set of program policy decisions.

ROBERT H. TRICE
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Perception and Misperception in International Politics *by Robert L. Jervis. Princeton, N. J., Princeton University Press, 1976.—xii, 445 pp. Cloth, $22.50; paper, $11.50.*

Robert Jervis has demonstrated once again his masterful occupation of that erstwhile no-man's-land between psychology and foreign policy. In his earlier book, *The Logic of Images in International Relations,* the point of departure was essentially how to convince (or deceive) others. This time he has turned the problem around: how not to deceive oneself—or unintentionally to mislead others. If the earlier book was the more original, that may be the consequence of the difference in these questions. The terrain of perception and misperception has been heavily worked over by psychologists; what it calls for—and what it receives in this book—is careful, thorough, imaginative application to the problems of state actors in their international political relationships. There is no better work on the perceptual dimension of foreign policy decision making.

The book has four parts. The first is in the nature of an introduction of a phenomenological inquiry. Its major concern is with the significance of the perception of intentions, and its major component is an extended analysis of the debate between deterrence theorists and spiral-model theorists. Having approached the barbed wire from this foreign policy side, Jervis then assaults it from the psychological side: the next three parts of the book deal with diverse processes of perception, with a variety of common misperceptions, and with a set of final observations on how decision makers might minimize misperceptions (e.g., read this book!). His chapters treat, *inter alia,* consistency theory, "evoked sets," attitude change, cognitive dissonance, and how decision makers learn from history. In the latter chapter, and in half as many words, Jervis so effectively supplements Ernest May's argument (in *"Lessons" of the Past*) as to displace it entirely. This piece alone is worth the price of admission.

It is impossible, in reviewing a book as meaty as this, to do it either descriptive or critical justice in necessarily short compass. One must leave countless lesser gems unappraised (e.g., his discussion on p. 310 of the positive consequences of the failure of top decision makers to get involved in issues early), and lesser nits unpicked (e.g., his recurrent difficulty—pp. 145, 172, 177, 406—with the slippery problem of whether varied ways of handling discrepant information are "rational"), to concentrate instead on two general issues.

1. In his ambitious attempt to organize and interpret the relevant psychological literature, Jervis unavoidably creates the impression that the varities of actors' foreign policy perceptions are, if not infinite, at least enormous—an impression that does not quite square with the amount of regularity and predictability we can observe in interstate behavior. He is well aware of the problem; yet in justifying and developing a phenomenological approach he has chosen to underplay the extent to which institutional and political processes may constrain individual perception and/or behavior. He has transferred insights and findings from the perceptual psychology of individuals rather directly to foreign policy decision makers (and on several oc-

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casions even to states-as-actors—e.g., Chapter III, *passim*, and pp. 246 and 249), without systematically considering the mediating effects of any institutional or political factors other than political incentives for accuracy in perception—and these only briefly. But he then illustrates and supports these propositions with historical examples many of which have political-institutional constraints built in, so to speak. While the range of his historical research is every bit as impressive as that of his psychological research, some reservations exist about the extent to which historical examples can constitute appropriate evidence for the psychological propositions in support of which they are adduced. Yet his dilemma can be appreciated: What other kinds of data are there? We make do with what we have—but caveats are in order.

2. What is missing (and hopefully will be the subject of his next book) are the threads that connect the diverse constituent mechanisms of perception. We learn that perceptions are a function of such things as images, deeply rooted expectations, evoked sets (i.e., immediate concerns), predispositions (drawn in turn from diverse sources); but it is not at all clear which of these perceptual mechanisms in any given actor can be expected to rule, or even to come into play, in what circumstances, or even whether they will interpret a common set of stimuli in mutually consistent or inconsistent ways. Jervis has put forth a rich and complex set of possible mechanisms, and has explored them as fully as present understandings permit. The next and more difficult step, for those who would close out this particular no-man's-land forever, is to make of them a theory.

BERNARD C. COHEN
University of Wisconsin—Madison

The Last Chance: Nuclear Proliferation and Arms Control *by William Epstein. New York, The Free Press, 1976.—xxiv, 341 pp. $14.95.*

Nuclear proliferation, the spread of nuclear explosive weapons into the arsenals of additional countries, is clearly bad. It can increase the likelihood of wars, and it almost certainly will increase the death and destruction in such wars. Research analyzing the prevention of such proliferation will thus be altogether topical.

There is a great deal of useful information and analysis in this book. William Epstein worked on arms control matters for twenty-five years with the United Nations, and he presents a clear, comprehensive and evenly balanced discussion of the various aspects and ramifications of the nuclear proliferation problem. This may indeed serve as a textbook in any course on contemporary arms control problems, for the chapter layout is quite explicit and readable.

What disagreement one might have with the author thus will not hinge on the factual accounts, but rather on issues of stress, on how best now to contain the proliferation problem. Epstein makes the sensible point on page 283 that hopes for arms control will probably have to be pursued independently of grander hopes for

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world government. Yet he does not take the further step of considering whether the prevention of proliferation should not be pursued quite independently of other arms control. The tone of the book throughout thus tends to link failure in stopping "horizontal" proliferation with failure in stopping "vertical" proliferation (i.e., other arms races). This becomes particularly evident in Chapter 18 on the "failure" of the 1975 NPT Review Conference; he accepts very literally the grumblings of the diplomats of the "Group of 77" that the superpowers' failure to make meaningful progress at SALT was relevant to their own obligations to forgo nuclear weapons.

It is not so clear that there is a true linkage here, or even that countries like Mexico and Nigeria and the Philippines really see a linkage. (Their diplomats of course have to pretend to see a linkage). The continued superpower arms race is bad, and continued nuclear proliferation is bad, but the two may be handled better separately than jointly. The author obviously does not see it that way.

Linked to this overall premise of the book, one thus sees less discussion than might be appropriate to the special nuclear proliferation temptations of Israel and of South Africa, and of South Korea and Taiwan, all countries close to the bomb, all countries likely to be totally indifferent to Soviet-American progress on SALT. The political ramifications of nuclear proliferation in these cases simply do not fit the unitary picture of arms control that Epstein endorses.

The author similarly devotes relatively little discussion to the selfishly unilateral options that the United States and Canada and like-minded nations will now have for the forestalling of nuclear proliferation, through the coordination of the Nuclear Suppliers' Conferences, or the stifling of plutonium reprocessing schemes and new uranium enrichment processes. It is here where some of the tougher choices for an antiproliferation strategy will have to be made.

A final kind of unitary bias is shown in the very title of the book. It is indeed true that every addition to the nuclear club is a setback for world peace and international security. Yet the chance to stop number seven is not our "last chance," any more than was our mismanaged effort to stop India as number six.

GEORGE QUESTER
Cornell University

The Politics of Space: A Comparison of the Soviet and American Space Programs *by William H. Schauer. New York, Holmes & Meier, Publishers, 1976.—vii, 317 pp. $24.50.*

William Schauer has attempted a comprehensive account of the policy and administrative history of the United States and the Soviet space programs. His emphasis is rather more on the Soviet program, with American activities analyzed in counterpoint. Although the whole period of the "space age" is discussed, there is much more depth in the analysis of the organizational and political dimensions of the first post-Sputnik decade (1957–1966).

Extensive citations to the existing literature on space policy are both the strong

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and weak point of the book. Strong, because Schauer has surveyed a broad range of official documents and secondary literature, and his bibliography is a valuable guide to this literature. (From the evidence in the book, Schauer does not read Russian; he cites only English translations of Russian sources. This could be a rather serious shortfall in a book which purports to analyze Soviet motivations.) However, Schauer's use of published materials is not very insightful; he uses other's opinions rather indiscriminately (even Penovsky's), and seems to have neither a good "feel" for the realities of space policy nor an analytic perspective from which to sort out conflicting views about the motives underpinnning national investments in space capability. As a result, there is no overall argument to the book, but instead a series of rather disconnected chapters, each summarizing the existing sources on the topic under discussion. The space programs of the United States and the Soviet Union are fascinating national enterprises which could provide a rich basis for comparative analysis of how two very different societies go about similar tasks, but such analysis is lacking here.

Schauer's book is also both naive and dated. He claims that "to the extent that space research remains a dramatic endeavor for general publics, [it] will exert a powerful force weakening the divisions and strengthening the political bonds between the two countries" (p. 227). But large-scale space activity has been undertaken almost exclusively for nationalistic goals which are supposed to symbolize the superiority of a political/economic system, and there is no evidence that the conduct of such activity undercuts that set of motivations. It would surprise the managers of NASA over the past few years to find, as Schauer suggests, that "one function of American space programs is to find politically acceptable and rewarding ways of spending money, not saving it" (p. 201). This statement is an example of a fundamental weakness of the book; it is based on a citation to a book written in the mid-1960s, and here and almost everywhere else Schauer does not seem to recognize that much has changed in United States space policy since the peak of the Apollo program. Certainly NASA is no longer, as Schauer describes it, "a large and powerful agency with large funds to disburse" (p. 120). Such unrealistic statements cause great hesitation in accepting Schauer's analysis of the Soviet space program.

What Schauer has done, and that not well, is to summarize the policy aspects of the space programs of the early 1960s. His chapter "Motivations and Goals" deals exclusively with that period, though it is presented as if the impulses of 1957 and 1961 are still valid. They are not, and there is need for a perceptive analysis of how the United States and the Soviet Union have designed their space policies in the 1970s, when the strategic competition which spurred them into a "zero-sum game" in the 1960s has largely vanished and space spending must be justified on its own merits, not in its relation to some dramatic national goal.

Finally, the book has many annoying errors. For example, the dates for the beginning of Apollo (p. 164) and the first manned landing on the moon (p. 175) are wrong.

The Politics of Space adds little to the literature analyzing space policy. Although

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there is now a lull in high-visibility activity in space, within a few years space shuttles will be routinely operating between earth and orbit. It would be valuable to understand why that capability is under development, and what its implications are for man's exploitation and exploration of space during the rest of the century.

JOHN M. LOGSDON
George Washington University

The "Tar Baby" Option: American Policy Toward Southern Rhodesia *by Anthony Lake. New York, Columbia University Press, 1976.—xiii, 316 pp. Cloth, $17.50; paper, $5.95.*

Anthony Lake, the new head of the Policy Planning Staff at the Department of State, was director of the Special Rhodesia Project for Carnegie Endowment from 1972–1974. Out of this project came this book on American policy toward Rhodesia.

As Lake repeatedly points out, there was scarcely any policy at all. In focusing on the period from Rhodesia's unilateral declaration of independence in 1965 until mid-1975, Lake describes two American positions. There was early support of Britain—coupled with a desire to remain well in the background—which ultimately gave way in 1969–1970 to the Nixon-Kissinger strategy of improving "communication" with the white regimes of Southern Africa.

The book ends in mid-1975—that is, before the Angola crisis of 1975–1976 and before the major initiative taken by Secretary of State Kissinger on Rhodesia in 1976. This recent growth of American interest toward Rhodesia and Southern Africa has rather dated the study. However, we now have the unusual opportunity of observing a partisan and committed advocate of a changed United States policy toward Rhodesia in a position where he might be able to do something about it.

The thesis of this book is that Rhodesia "presents for the United States a classic case of forced choice between narrowly conceived economic gain and our broader national interest in the observance of international treaty obligations and the promotion of international concern for human rights" (p. 4). Lake then shows in great detail how the choice was almost invariably for economic gain—an easy choice because of public and political indifference about Rhodesia. In this type of situation Lake argues, lobbyists—particularly those who can tie their arguments to cold war/national security rhetoric—almost always get their way, especially when Congress is inattentive and the administration is unconcerned.

The book's strength lies in Chapters 3, 4, and 6. In Chapter 3 the policy situation from 1965–1968 is reviewed. Lake demonstrates that Rhodesia was seen as a British problem. George Ball is identified as the man most responsible for the decision of the United States to stay in the background and make as little noise as possible. Lake praises Ambassador Arthur Goldberg for "calculated and precise ambiguity" (p. 86) in 1965 when Goldberg helped ward off an early oil embargo against Rho-

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desia. Lake seems to believe that the Johnson administration's policy toward Rhodesia was weak and ineffectual, but he is rather reluctant to say so. Officially, the policy was seen as "successful" (p. 120).

In Chapter 4 the shift to the "tar baby" is analyzed. The image comes from the Uncle Remus story in which Brer Rabbit gets stuck to the tarred Brer Fox. The point is that in drawing closer to the white regimes after 1969, Nixon and Kissinger were getting stuck to an impossible policy. The Angola imbroglio certainly proved this point as even Kissinger was ultimately forced to admit (however obliquely).

Finally, in Chapter 6 there is an excellent discussion of the Byrd Amendment—the legislative effort of 1971—which opened the way for chrome and other materials to be imported from Rhodesia. Thus the United States joined with South Africa and Portugal as the only nations in the world to openly violate their legal obligations to support United Nations mandatory economic sanctions.

Lake is at his best as he chronicles the legislative twists and turns which made the Byrd Amendment possible, the bureaucratic infighting on this issue, and in his powerful arguments, especially in Chapter 7, about the consequences of this action.

With the Carter administration apparently far more concerened about human rights than its predecessors, we will hopefully be able to watch the end of a misguided policy. Those who support Lake's arguments here might look at his eight ways to work more effectively to change United States policy (pp. 282–285). And we can further hope that he will not fall victim to "one of the most terrible of bureaucratic gaffes: he made no secret of his beliefs . . ." (p. 65) which he claims made J. Wayne Fredericks—a former ranking official in the Bureau of African Affairs—ineffectual.

It should be noted that this is not really a scholar's book. There are no footnotes and only a perfunctory bibliography. Lake's claim to reliability lies primarily on extensive interviews carried out by himself and his research assistants. There are occasional errors. For example, South African Prime Minister Vorster is mistaken for the former Prime Minister Verwoerd (p. 80); South Africans become Southern Africans (p. 54); the date for the Harare bus boycott is wrong (p. 31)—it should be 1956.

But these are minor points. The book stands on its own and will be a useful reference guide to what policy changes can and should be made. As Lake points out, "whenever the American government does less than it can to meet its international legal obligation to observe the U.N. sanctions program, a final question is raised about American concern for international law as well as for the laws of our own society" (p. 197).

LARRY W. BOWMAN
University of Connecticut, Storrs

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Managing an Alliance: The Politics of U.S.-Japanese Relations *by I. M. Destler, Hideo Satō, Priscilla Clapp, and Haruhiro Fukui. Washington, D. C., The Brookings Institution, 1976.—x, 209 pp. Cloth, $9.95; paper, $3.95.*

Why do allies misunderstand one another? How can those who try to promote harmony simultaneously generate discord? Can one, by analyzing the past, identify the enduring political characteristics of an alliance so as to manage it more skillfully?

These questions are the rationale for this book's analysis of three cases of American-Japanese interaction: the security treaty revision crisis of 1960; the Okinawa reversion negotiations of 1965–1972; and the rancorous dispute over textile quotas during the first Nixon administration. After Clapp summarizes the facts of each case, other coauthors develop alternative explanations for strikingly different outcomes in the Okinawa and textiles cases. Destler and Fukui focus on institutional differences. They show how American failure to appreciate the effects of fragmentation of power, consensus orientation, and bureaucratic elitism in Tokyo rendered nugatory summit level promises to produce a solution to the textiles problem. In the next chapter Satō analyzes misperceptions. After distinguishing between those which are institutionally and culturally rooted, he summarizes the work of Japanese sociologists and anthropologists so as to highlight the particular pitfalls of negotiating with Tokyo. The final analytical chapter blames communication and negotiation techniques for alliance difficulties. It shows how asymetrical media coverage which transforms a minor incident in Washington into a major news story in Tokyo complicates alliance managers' tasks. The discussion of the follies of simultaneous diplomatic, functional, summit, and back-channel negotiation helps one comprehend how even well-intentioned elites can misunderstand one another.

The authors conclude with a sermon for American alliance managers. They hand down more commandments than those given to Moses and, like Jesus, reduce them to two: Be sensitive to Tokyo's domestic and bureaucratic politics; and never forget that presidents and secretaries of state have values and goals more important than preserving good American-Japanese relations.

One can use and assess this book on three different levels. It does present valuable new information about the Okinawa and textiles disputes. It distills the best of recent methodological advances in the study of international relations and combines this with the insights of Japanologists. In clear, concise, and consistent language the authors present and apply the theories of Allison and Neustadt, Jervis and Steinbruner, and Keohane and Nye. These qualities make the book attractive for classroom use.

But on another level, scholars are likely to be dissatisfied. Even if one accepts the disclaimer that the case studies are ancillary rather than central to the authors' purposes, it is difficult to avoid the conclusion that they are presented inadequately. Greatly condensed and denuded of source citations present in their original form, the cases become mere sketches in the book. Important facts central to the narrative are separated from it and buried in a thicket of other explanatory examples in the

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analytical chapters. This creates the impression that too many questions have been posed and too many scattered, incomplete answers offered.

Finally, readers will judge this volume in terms of its policy prescriptions. While they constitute good advice for any alliance manager, are they also sufficient and practical? Creating a subcabinet-level monitor for relations with Japan and other major allies might make official Washington a bit more sensitive to politics in other capitals. But would this be in tune with the populist style of the fledgling Carter administration? Would so narrowly an elitist approach be consistent with recent efforts, manifest in the passage of the U. S.-Japan Friendship Act and in the activities of the Japan Foundation and the U. S.-Japan Cultural Commission, to broaden the base of popular understanding between America and Japan? Whatever the answers, one is certain to find this book stimulating and provocative of fresh thoughts on alliance politics.

ROGER DINGMAN
University of Southern California

The Hovering Giant: U. S. Responses to Revolutionary Change in Latin America *by Cole Blasier. Pittsburgh, University of Pittsburgh Press, 1976.—xix, 315 pp. Cloth, $15.95; paper, $4.95.*

Cole Blasier sets out to "describe, compare, and explain" (p. 4) United States responses to four main cases of "revolutionary change" in Latin America: Mexico (1910–1940), Bolivia (1943–1964), Guatemala (1944–1954), and Cuba (1956–1961). An Epilogue surveys later cases in the Dominican Republic, Peru, and Bolivia.

This is, in many ways, an extremely satisfying book. It is well written, insightful, balanced in interpretation, and superbly documented primarily from American and German archives. Though his book is diplomatic history at its best, Blasier's central aim is that of a political scientist, generating "propositions" about "official behavior."

Blasier identifies three stages of revolutionary change: rebel movement, reformist government, and revolutionary government. In each of the four main cases, except Bolivia (where Washington feared Nazi influence over the Movimiento Nacionalista Revolucionario [MNR]), United States decision makers responded with some flexibility to rebel movements. Serious frictions emerged in the second stage when reforms threatened private American interests. Ultimately, in the third stage, the United States either negotiated a settlement (in Mexico and Bolivia) or opted for suppression (in Guatemala and Cuba).

According to Blasier, American policy in these cases was principally governed by strategic concerns, preventing or countering the interference of Germany before 1945 and the Soviet Union thereafter. However, private business and other economic interests "were important in the formulation of U. S. policies and were often com-

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patible with policies decided primarily on strategic grounds" (p. 236). Strategic factors definitely took precedence in the Mexican case, when American decision makers eventually attached less importance to private claims than to keeping German influence out and avoiding conflict with a neighboring government while war loomed overseas. In general, at times when strategic concerns were less pressing, private interests came to the fore.

Bureaucratic factors, although present in all cases, were less significant than strategic or economic ones. The bureaucratic dimension was most prominent in Bolivia after 1952, as certain United States officials came to identify personally with the goals of the MNR revolution. As a rule, lower-level decision makers tended to be stronger defenders of private interests than presidents or secretaries of state, whose authority customarily was required to make decisions strongly opposed by private interests.

In Blasier's view Washington was "essentially correct" about German influence in Mexico and Soviet influence in Cuba, but misread the degree of German and Soviet influence in Bolivia and Guatemala, respectively. Accommodation had a "moderating and deradicalizing" impact in Mexico and Bolivia and thereby served American interests, while repressive policies in Guatemala and Cuba "tended to polarize those societies and radicalize opposing groups" (p. 238). United States pressures against Cuba, Blasier argues, pushed Castro ever closer to the Soviets and escalated dangerously into the Cuban missile crisis. He comments: "The United States has often brought on itself what it most seeks to avoid, the interference of rival Great Powers in the hemisphere. In this sense, the United States has long been its own worst enemy in Latin America" (p. 273).

Blasier regards later cases as largely consistent with previous experience. Preventing "another Cuba" was the main factor behind United States intervention in the 1965 Dominican civil war. Concern about Soviet penetration declined as the decade wore on, which, with the Peruvian military's self-professed anticommunism, made it possible to reach an understanding of sorts with the Velasco government. However, in addition to a broader gamut of private interests, strategic concerns again came into play in the Chilean case: Allende was perceived as threatening to the traditional United States sphere of influence and as a symbol of the success of the peaceful road to power that might be emulated by socialist movements elsewhere.

In conclusion, Blasier urges the United States henceforth to accommodate to Latin American change that is to some extent inevitable and to "maintain a certain distance" from investor disputes in order to encourage negotiated settlements.

YALE H. FERGUSON
Rutgers University-Newark

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America's Moment: 1918, American Diplomacy at the End of World War I *by Arthur Walworth. New York, W. W. Norton & Company, 1977.—vii, 309 pp. $14.95.*

Arthur Walworth is a tireless researcher. Yet *America's Moment: 1918*, by the author's own intention, is an unfinished work. The jacket blurb informs readers that Walworth is completing the sequel which will deal with American diplomacy at the Paris peace conference. Until that book is published, comments on the prologue and the *mise-en-scène* run the risk of doing the author an injustice. Which themes will be developed; which left to stand as they are? So far, however, Walworth has set out the origins of American planning for the peace, reminded us of the complexities and contradictions in Woodrow Wilson's world view, and given his readers a nicely wrought picture of Allied reactions to American "intervention." There is, in addition, a good deal of "filler" in the book, which stretches *America's Moment* to probably unnecessary length. Fortunately, Walworth's prose smoothes the way, so the journey to Paris never becomes tedious.

Discussing Wilson's decision to go to the peace conference at the head of the American delegation (really the *only* American delegate as it turned out), Walworth presents the pros and cons with equal force and equal judiciousness. Colonel House, who opposed the decision apparently out of fear that the president would squander *his* "victories" in preconference negotiations with the Allies, emerges (as in earlier treatments) as the realist, the necessary go-between for Old World statesmen and the American prophet.

House had a lot of explaining to do in this role, because his leader's words seemed capable of translation into no language known to diplomats. Hearing Wilson disclose what the president called "the real inside of his mind," a French journalist concluded that his vanity no longer recognized any limit. He apparently thought he could "play professor for a world empire" (pp. 14–15). Even so, David Lloyd George refused to allow Wilson to occupy alone the moral heights overlooking the rest of God's creation, and sought to match the new world prophet speech for speech, and ship for ship.

Wilson chose odd moments for revealing his mind. One of the least successful, Walworth notes, was on the steps of the French Embassy in Washington, where he expressed great satisfaction that *he* had restored, or would restore, Alsace-Lorraine to France. On board the *George Washington* on the way to Paris, Wilson became even more boastful (and less than discreet) around newsmen who would print stories of the president's attitude as the American crusade made its way to unenlightened Europe. Curiously, Wilson would reverse himself on important points at equally odd times. He had held, for example, that Germany must be admitted to the new association of nations as a founding member. But once in Europe he changed his mind. Walworth suggests no reason for this abrupt turnabout, but merely records that when the king of England queried him on the point, Wilson said that German entry would depend upon its future behavior.

Wilson often ruminated about Germany. On one occasion he suggested to the

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French ambassador that a postwar Hohenzollern Germany would not be strong, whereas a Bolshevist Germany would be incapable of paying reparations. What Ambassador Jusserand made of these ramblings, Walworth does not say. Wilson's deep concern about bolshevism is well treated, with the single (but important) exception of the president's rationale for sending troops to Siberia. For some reason Walworth omits from the list of considerations Wilson's desire "to steady any efforts at self-government" that might spring up in Siberia.

Few scholars have paid adequate attention to the crucial self-contradiction in Wilson's appeal to the "imperative principle" of self-determination as against his insistence upon an effective universal government. Ironically, it was Lenin who seized upon self-determination to protect his government from intervention. Once the peace conference began, it was too late to rethink that and many other problems.

LLOYD C. GARDNER
Rutgers University

The Transformation of American Foreign Relations, 1865–1900 *by Charles S. Campbell. New York, Harper & Row, 1976.—xviii, 393 pp. $15.00.*

The United States of America in 1865 was avowedly anticolonial, expressedly Anglophobe, overtly isolationist. American foreign policy seemed to be predicated on an indifference to world affairs and to be concerned with very limited international diplomatic goals. Within a comparatively short time the United States became actively imperialist, implicitly Anglophile, and much more concerned with international developments. By the turn of the century the foreign policy of the United States (which now obviously was becoming a world force) was directed toward increasing the nation's international power and prestige. In this splendid volume, Charles Campbell interestingly and convincingly chronicles how and why "a country not under duress experienced, in so short a time, so radical a transformation of its relations with the rest of the world" (p. 337).

Campbell's work, part of The New American Nation Series edited by Henry Steele Commager and Richard B. Morris, is built around the twin themes of Anglo-American relationships between the end of the Civil War and the turn of the century, and the abortive as well as successful attempts at American territorial expansion overseas during these years. He quite correctly defines these themes as "the two main foreign policy issues throughout the years dealt with in this book" (p. 5). In dealing with these issues Campbell demonstrates considerable familiarity with the secondary literature and makes very intelligent use of it and of a wide variety of primary sources. He builds in first-rate fashion on the groundwork laid by other scholars such a William Appleman Williams and Walter LaFeber as well as by his own writings on American foreign policy.

The author delineates various factors as being responsible for the transformation which is ascribed to American foreign policy. Among them he includes the various

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cultural and social ties between the British and American upper classes, the ardor and influence of the religious missionaries (especially those involved with China), Social Darwinism and the racism that became known as Anglo-Saxonism, the geopolitical ideas of Captain Alfred T. Mahan (with their emphasis upon seapower), the rapid settlement and economic development of the American West (especially the Pacific Coast), and the metamorphosis of the United States from an agricultural to an industrial nation. Campbell understandably does not give equal weight to all these factors, and he stresses the changes that take place in the American economy, particularly in the 1890s when "many Americans came to wonder whether exports could be increased significantly without the acquisition of colonies, which would not only . . . themselves provide markets but . . . would serve as points of strength for assuring access to larger markets . . ." (p. 143).

For all his flair in dealing with such disparate themes, Campbell writes ponderously. His prose is never eloquent and at times his style lets him down badly. Moreover, although stressing the importance of American economic developments he concentrates on changes in industrial output and neither challenges nor dismisses the theory put forth (most notably by Williams) that the quest for markets by commercial American agriculture played a significant role in territorial expansion by the United States in the 1880s and 1890s. Campbell also stints on analysis. Detail is never lacking, but while the bricks are all there, he might have supplied some more mortar.

But these caveats are far outweighed by the positive qualities of the book—including well-chosen illustrations, among which are a fine selection of cartoons from the press of the day, and a thirty-eight-page bibliographical essay which is comprehensive as well as sophisticated and carefully thought out. *The Transformation of American Foreign Relations* is first-rate, maintaining the New American Nations Series' well-deserved reputation for high quality and general usefulness.

DANIEL J. LEAB
Seton Hall University

The New Presidential Elite: Men and Women in National Politics *by Jeanne Kirkpatrick. New York, Russell Sage Foundation and Twentieth Century Fund, 1976.—xix, 605 pp. $20.00.*

Jeanne Kirkpatrick has written the most extensive study of delegates to national nominating conventions since the David, Goldman, and Bain studies of the 1950s. Basing her conclusions on data from 1300 interviews with delegates to the two 1972 conventions and on responses to mailed questionnaires returned by over half of the delegates, Kirkpatrick describes in depth the backgrounds, attitudes, orientations, and roles of this particular political elite. She justifies the concentration on this group because of a basic assumption that "delegates to the two conventions constituted a cross section of the effective elite of the presidential parties and that

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for this reason they constituted a promising perspective from which to observe the American political scene" (p. 18).

In fact, two rather separate studies are contained herein. In the first eleven chapters, Kirkpatrick deals with the entire body of convention delegates and the implications of her description of these elites for presidential politics. In the last three chapters, she turns specifically to the women delegates, seeking to define the meaning of their emerging prominence. The two sections seem connected only by the data base; they could well have been separate works. Each, however, makes important contributions to the literature on political parties, and each has certain weaknesses which should be noted.

The main portion examines what Kirkpatrick calls the "new breed" hypothesis, namely that among the delegates to the 1972 convention many were distinguishable from those who had dominated earlier conventions in very basic ways and that these differences had important implications for the future of presidential parties. Kirkpatrick argues that, because of the new rules in effect in 1972, conventions are particularly open institutions and that "permeability permits new generations to move rapidly into leadership positions, permits outsiders to move in, and generally accelerates the circulation of those in power" (p. 38). She views this situation with some alarm because the new breed which dominated the Democratic convention in 1972 was ideologically removed from the mass of Democratic voters and, even more important, was committed neither to the party as an institution, nor to the goal of winning the general election. "*Any deemphasis on winning elections would profoundly alter the fundamental character of the American parties by removing the 'discipline of the market.'* . . . Though the reform of the Democratic party was accompanied by repeated references to the desirability of more responsible parties, in retrospect it is clear that those reforms significantly weakened that party" (pp. 353–354).

The difficulty with this analysis and with much of the early part of the book results from overreliance on the survey results and too little attention to the dynamics of nomination politics. Kirkpatrick is an activist in the moderate wing of the Democratic party; her analysis of 1972 reflects a Humphrey supporter's view of the effects of the McGovern-Fraser reforms. It is a well-documented analysis, but hardly free from bias. Viewed from after the 1976 nomination, Kirkpatrick's fears seem ill founded. Nothing in her analysis would have led one to believe that those interested in recapturing the White House could have used the permeability of the convention to the same effect as the liberals did in 1976; nothing would have accounted for the role played by Robert Strauss from 1973–1976; nothing would have predicted the capture of the 1976 convention by supporters of Jimmy Carter, based on an appeal far different from that of the "new breed" in 1972.

Similarly, Kirkpatrick's discussion of the role of women in the 1972 convention makes important contributions but is flawed because she does not fit her analysis into the politics of the convention. Her most interesting finding is that "*on most major issues of public policy, including those concerning education and the poor, the views of women delegates were virtually identical with those of men*" (p. 439).

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However, she downplays the one major exception to this—views on military policy—which were vitally important in 1972. More significantly, she does not adequately distinguish the views of the most active and vocal women. Kirkpatrick emphasizes that women were rarely perceived to be among the most influential members of state delegations, but she does not discuss the potentially critical role of the Women's Caucus. Because of this omission, she does not deal with the potential changes in the internal politics of conventions, caused by the increasing numbers of women, which have been perceptively noted by Denis Sullivan, Jeffrey Pressman, and their associates.

Kirkpatrick correctly points out the need to study the Republican as well as the Democratic party. Too much of our recent literature has dealt with the Democrats, mainly because more has been happening in that party. Unfortunately because of the nature of the 1972 nominating process and because of the hypothesis she tests, Kirkpatrick also spends much of her time dealing with the Democrats. She presents data on the Republicans and draws comparisons but does not present an in-depth analysis of these findings. One would hope that more work on the Republican party will come from this project.

None of these points detract from the importance of this book. In a most scholarly manner Kirkpatrick relates her findings to the entire history of theoretical and practical literature on parties. The data she presents are nearly overwhelming. While some problems of interpretation do exist, as a study of the delegates to the nominating conventions surely this book will be the standard to be matched for some years to come.

LOUIS MAISEL
Colby College

Changing Campaign Techniques: Elections and Values in Contemporary Democracies *edited by Louis Maisel. Sage Electoral Studies Yearbook, Vol. 11. Beverly Hills, Calif., Sage Publications, 1976.—274 pp. $7.50.*

According to the publisher's back page blurb, the Electoral Studies Yearbook "will focus on a central theme, and carefully prepared *original* chapters will develop that theme fully, from both cross-national and interdisciplinary perspectives." And the present volume imposes even more stringent demands upon its contributors and concomitantly raises the readers' expectations. For it claims to be "devoted to the detailed study of the consequences" of such technological changes as "high-speed low-cost printing, television, and the adaptation of polling and computer technologies . . ." as well as "their impact on democratic political processes" (back page). The claim is scarcely fulfilled. Indeed, the yearbook does not consider in any coherent, systematic way how campaign techniques are changing (if they are), or the observable effects of such changes (if there are any). Instead it provides a hodgepodge of articles of disparate quality, which must be scrutinized carefully for their contributions to the subject.

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There are three articles on the public financing of elections. Joel L. Fleishman, drawing on and advancing from his law review publications, marshals some of the constitutional, philosophical, and logical arguments in defense, variously, of the principle of public financing, possible legislative provisions, and the actual law and amendments enacted. He is followed by Jewel Bellush and William J. D. Boyd who, in a spirited attack, argue that subsidizing elections with public funds generates such problems as "interfering with civil liberties, protecting incumbents and blocking insurgents, encouraging anti-party politics, inducing the phony campaign, curbing the minor party challenge, linking expenditures and electoral success, inflating the price tage [*sic*] and generating false expectations" (p. 56).

Both articles are more polemical than analytical. One of the few direct clashes exemplifies the level of discourse. Bellush and Boyd argue that one effect of the public financing proposals is to make many Republicans contribute funds to Democratic candidates, in fact more Democratc will be supporting Republican dandiception. While the value of Republican taxes paid may be a higher proportion of net tax proceeds . . . the *number* of Democratic taxpayers . . . is much higher . . . and since the check-off funds are evenly divided between Republican and Democratic candidates, in fact more Democrats will be supporting Republican candidates [*sic*] than vice versa" (p. 37). This is reverse corroboration, not refutation.

The editor then speculates about the effects of the 1974 public financing amendments on minor party candidacies in 1976 and thereafter. He foresees "a situation in which the major parties continue to dominate the political scene but in which minor parties—certainly one or two on the right, perhaps one on the left, some weak, some strong—become a permanent and influential feature, a two-plus-n party system" (p. 88).

The section on public financing would have been improved had the contributors been familiar with and directly responded to each other's arguments (Fleishman appears to have seen a 1974 version of the article by Bellush and Boyd, while they seem unaware of his work); had they been able fully to incorporate into their articles the U. S. Supreme Court's *Buckley* v. *Valeo* (1976) decision upholding most of the Campaign Financing Act; and had the editor tried to reconcile the conflicting arguments.

The remaining article on American politics is an attempt by Paul A. Dawson and James E. Zinser to explore "the extent to which congressional candidates are cognizant of, or constrained by, their environment in making campaign resource allocation decisions" (p. 128). They find that "three variables—per capita income, income inequality, and mobility—are particularly successful in 'explaining' variations in expenditure behavior" (p. 129). Granted this is a surprisingly unresearched subject, but the article is arid and far too long for its few insights.

The rest of the collection is a European tour. Alastair Davidson provides a fascinating capsule history of the Italian Communist party's efforts to achieve independence from the USSR and to reduce its own Leninist structure, as it has developed its policy of "historical compromise." This is followed by a translation of a chapter from Alfred Grosser's *La Politique en France* which is hardly related

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to campaigns, let alone their techniques, and does not appear directed at an academic audience.

The final three articles deal with elections in the United Kingdom and are most immediately germane to the yearbook's subject. Anthony Smith offers some characteristically unsystematic, informative, and insightful musings about television coverage of the 1974 general elections. Victor J. Hanby traces the rise, fall, totter and rise of the Scottish National Party. He tells us that "the intensive retailing of SNP themes and imagery by the party campaign machinery together with their consequent swift success and influence on the population, should at the very least warn as to the possibilities for excess and manipulation inherent in the panoply of contemporary campaign practices, techniques, and modes of presentation that dominate present-day elections" (p. 237). Then, in the final article, E. J. Dione, after describing campaigns in several constituencies during the 1974 British election warns us not to exaggerate the changes the new campaign technology have wrought in British politics and certainly not "to overlook crucial continuities" (p. 265). A final chapter of analysis, clarification, and reconciliation, suggesting directions for future research, is needed but not provided.

DAVID L. PALETZ
Duke University

The First Amendment and the Future of American Democracy *by Walter Berns. New York, Basic Books, 1976.—xi, 266 pp. $12.50.*

Walter Berns, professor of political science at the University of Toronto, is one of our leading constitutional law writers. His scholarship is wide ranging, deeply rooted in historical sources and moral values, and he writes vigorously, clearly, and responsibly.

Those who are familiar with Berns's previous writings will find in this book certain basic themes with which his name is now firmly linked. His overall thesis is "that the Court, in the name of civil liberty, is steadily eroding the conditions of civil liberty, to the point where it is appropriate to wonder about the future of liberal democracy in the United States" (p. x). More specifically, he believes that the Supreme Court has been misconstruing the constitutional prohibition of an establishment of religion, arguing that those who wrote the First Amendment did not intend to forbid state financial aids to religion so long as the aids are extended in a nondiscriminatory way. More importantly, he maintains that state aids to religion are necessary for the moral health of the nation. On this point he is less than persuasive. In addition, he insists that the Founding Fathers did not believe in an absolute freedom of speech, and that the Court in recent years has moved too far in that direction. He would, for example, not extend free speech to those who would destroy free speech if, through speech and association, they ever attained the power to govern. While he does not say so explicitly, the logic of his

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argument would justify—indeed require—the outlawing of the Communist party, as well as other groups espousing totalitarian objectives.

Furthermore, Berns draws a sharp distinction between "freedom of speech" which the Constitution protects, at least up to a certain point, and "freedom of experession," which he regards as an impermissible judicial extrapolation. Finally, since he believes that the state must protect "the conventions of decency" (p. 193), he is very critical of the Court's decisions on the subject of obscenity, decisions which he thinks are undermining the foundations of constitutional government. While this is highly debatable, he makes an appealing argument that while vice cannot be eradicated in a liberal state, at the least it ought to be made as difficult of access as possible.

Many of Berns's arguments are debatable, for example the two items discussed below. It was startling to read that the second flag salute case, *West Virginia Board of Education* v. *Barnette*, 319 U. S. 624 (1943), was decided on free speech and not on freedom of religion grounds (p. 39). While it is true that in the opinion of the Court, Justice Jackson used free speech language which was broad enough to cover a student who objected to the flag salute on nonreligious grounds, it is equally true that he made it clear that the complaining parties were asserting the freedom of religious beliefs (319 U. S. 629). Furthermore, there is explicit language in the concurring opinions of Justices Douglas and Murphy, and in the dissenting opinion of Justice Frankfurter, indicating that what was at stake was the "scope . . . [of] freedom of religion," or "freedom of worship" (319 U. S. 643, 645). Apparently Berns was unduly swayed by his eagerness to make the point that one cannot refuse to obey the law on religious grounds.

The second item is the author's discussion of seditious speech as it relates to First Amendment interpretation. He is devastatingly critical of Justice Holmes, describing him as "a dilettante who dabbled in philosophy" (p. 161), and argues that *Gitlow* v. *New York*, 268 U. S. 652 (1925) was not decided on clear and present danger grounds (p. 156). In the opinion of the Court, Justice Sanford wrote: "That utterances inciting to the overthrow of organized government by unlawful means present a sufficient danger of substantive evil to bring their punishment within the range of legislative discretion is clear" (268 U. S. 669). This is clear and present danger rhetoric, although quite different from the Holmes-Brandeis version, a difference which Berns ignores. What the Court said in *Gitlow*, and repeated in *Dennis* v. *United States*, 341 U. S. 494 (1951), was that a legislative determination that certain types of speech pose a clear and present danger was entitled, like all statutes, to the normal presumption of constitutionality. Thus Berns is simply wrong in asserting that in *Dennis* the jury was instructed to convict "only if they are convinced that an advocacy of forceful overthrow is not too remote from an attempt to overthrow . . . (p. 177). Actually, in his instructions to the jury Judge Medina declared: "I find as a matter of law that there is sufficient danger of a substantive evil that the Congress has a right to prevent to justify the application of the statute under the First Amendment . . ." (341 U. S. 512). As Justice Douglas pointed out in his dissenting opinion, a major objection to the *Gitlow-Dennis*

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version of the clear and present danger test was precisely that it did not leave much for the jury to decide.

It is true that clear and present danger language is rarely used these days by the Court, and that the line between licit and illicit speech usually turns on a balancing of competing interests. From this point of view, Berns's preferences—for republican self-control, decency and civility—are not to be taken lightly. Just where he would draw the line is not clear, but it is clear that he wants to draw the line at some point which would protect humane values. One can hope that he will continue to try to explain with more particularity just where his First Amendment philosophy would take us.

DAVID FELLMAN
University of Wisconsin-Madison

The American Police State: The Government Against the People *by David Wise. New York, Random House, 1976.—437 pp. $12.95.*

A government that uses its intelligence and police apparatus to violate established individual and societal rights is a terror to behold. More malignant to democratic thought is that government which not only misuses its great power but, in the process, loses effective control of the tools it illegally employs. Such a government is portrayed by David Wise in his third major book to explore the morals, methods, and machinations of an American administration in a bizarre relationship with an assortment of ruthless place seekers, intelligence professionals, and a menagerie of criminal types.

Wise is at his experienced best reporting on the labyrinthine "who did what to whom" aspects of the Nixon presidency. His character portrayals, sharp and perceptive, dispel any lingering belief that there truly was an imperial presidency. Emerging as petty, paranoid, incompetent, and strangely childish, the president's men are to be feared not for who they were but for the power they wielded.

Two key figures appear, not for the first time, in unflattering poses. Henry Kissinger is shown both to have known more than he cared to admit about illegal wiretapping and to have cared too little about illegalities he at best was aware of and at worst instigated.

About General Alexander Haig, whose role in the White House seems to have been that of a jack-of-all-tricks, responsible for none, many questions remain. Clearly he knew about many wiretapping and other criminal acts; just as clearly Wise has not ascertained his responsibility for them.

Wise's analysis of the organizational and legal intricasies of the emerging police state he describes so well is somewhat superficial. Too much attention is focused on the major personalities of Watergate and its aftermath, too little is devoted to identifying and understanding the role of the cold war intelligence warriors who formed the indispensable backdrop for the great scandals. An equally serious omis-

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sion is the failure to discuss the role of Army Intelligence in the police state table of organization.

The present senior intelligence community is comprised largely of middle-aged men, and a few women, who cut their operational teeth on the real and imagined Communist challenges of the post-Potsdam world. Secure and secreted—physically, emotionally, and intellectually—from the mainstream of American life, career intelligence personnel survived changes in administration and upheavals in society. Essentially functioning on a crisis-intervention model developed to counter foreign threats, the intelligence community had neither reason nor ability to resist the increasingly questionable demands made upon it by government. Indeed, encouraged by the prospect of useful domestic work, the community developed its own initiative.

A great impetus for the employment of intelligence personnel and dubious activities, recognized but not fully developed by Wise, was the absence of clear legal guide lines, a situation that has changed little since Watergate. Break-ins are illegal under any circumstances, but effective, statutorily mandated control of the intelligence services and the executive branch, with regard to investigations and related activities, would have done much to curb incipient official lawlessness. Perhaps the absence of such controls was irrelevant as far as Richard Nixon and his immediate followers were concerned. It is likely, however, that a legal foundation for overseeing the intelligence community would have acted as a brake preventing many of the excesses which are inseparable from the larger story of Watergate: the vast nationwide surveillance operations, the unlimited use of informers, and the creation of data banks of dissent.

In substantially omitting the military from his book, Wise weaves an incomplete tapestry. For more than a decade the Army, despite its denials, was an increasingly energetic and willing member of the domestic intelligence community. In scope if not sophistication, its operations spanned the nation, from ghetto to campus, and its massive output went to the highest officials. Until public disclosure made the Army's frequently illegal activities a political embarrassment, the White House encouraged unconstitutional military involvement in civilian affairs.

Wise's book is valuable. The imperial presidency concept is not rejected so much as it is shelved. The intelligence community has not lost its capacity to wreak havoc domestically. It is simply restrained. David Wise has pungently captured the persons and events who turned a democracy upside down.

RALPH M. STEIN
Pace University School of Law

The American Judicial Tradition: Profiles of Leading American Judges *by G. Edward White. New York, Oxford University Press, 1976.—xii, 441 pp. $15.95.*

Inspired by the late Richard Hofstadter, Professor Edward White of the University of Virginia Law School undertakes to define and illuminate an American "judicial,

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as distinguished from a political, tradition" (p. vii) through individual and group portraits of some of its most influential or representative shapers. White confines his conception of judicial tradition to the style and substance of appellate judging, which he approaches from the conventional perspective of analysis and evaluation of the concepts and techniques of legal reasoning. Twenty-one Supreme Court justices and seven state or lower federal appellate judges have been selected for study. While there will always be arguments about just who does and does not belong in judicial pantheons of this sort, White's choices are on the whole sound and appropriate to his purpose.

The essence of judicial office for White is its quality of being "independent but constrained" (p. 371); unlike the political branches, the judiciary is under a continuing obligation to stress its own limitations, in order to justify its exercise of undemocratic power. In the nineteenth century, White argues, judges believed and asserted that their function was limited to the discovery of preexisting law. The appropriate method of opinion writing was to reason from first principles, and the acknowledged master of this technique was of course John Marshall. Not surprisingly, White's profiles of the leading judges of that era belie the validity of this "oracular theory of judging" (p. 2), and the law emerges as a pliant and effectively result-oriented tool at the hands of such "oracles" as Marshall, Kent, Taney, and Field.

The discrediting of the oracular theory around the turn of the century precipitated a crisis of legitimacy, White maintains, and thus the central theme of the tradition since then has been the search for alternative defenses against the "vulnerability of the autocratic judiciary in a democratic society" (p. 252)—defenses that would justify continued judicial independence by erecting new formulations for judicial constraint. White's focus here is on the efforts of various judges to employ sociological, realist, and process jurisprudence to construct new and acceptable theories of the judicial function. His handling of these personalities and issues is notably more deft and stimulating than his rather orthodox treatment of those of the preceding era. Especially in his profile of Jackson and in three group portraits (Holmes and Brandeis; Cardozo, Hand, and Frank; Frankfurter, Black, Warren, and Harlan), White interweaves diverse strands of judicial character and philosophy with considerable finesse.

White's genre imposes its difficulties, chief among which is a certain imbalance of emphasis. The profiles tend to loom larger than the tradition itself, as White's two synthesizing chapters are regrettably slender. Moreover, the juxtaposition of the biographical and historical levels of analysis is disarmingly facile. White emphasizes social context but neglects that intervening medium of clashing interests, contentious advocacy, and judicial bargaining—the decision-making process itself—through which individual personalities, philosophies, and actions must pass before suffusing into tradition. Some large questions remain unexplored—such as the degree to which the oracular theory actually constrained those who believed in it—and some questionable judgments are merely asserted. For example, White states that Marshall believed the meaning of the Constitution should not change over

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time. He later reasonably attributes that same belief to the Four Horsemen of the 1930s, but whether they are thus Marshall's true legatees is another unexplored question.

On the whole, however, this is a valuable book. Laymen will find that much is assumed, while scholars find that much is familiar, but both can benefit from White's impressive integration of a large body of biographical, historical, and jurisprudential materials.

JOHN B. TAYLOR
Washington College

The Right and the Power: The Prosecution of Watergate *by Leon Jaworski. Pleasantville, N. Y., Reader's Digest Press, 1976.—305 pp. $9.95.*

"There had to be more to Watergate than what met the eye" Leon Jaworski thought to himself just before taking a phone call from Alexander Haig to discuss the possibility of becoming special prosecutor (p. 2). And there is more to the prosecution of Watergate than is contained in this account. Jaworski, an advisor to presidents and governors, and a leading member of several national bar organizations, has written a defense of his conduct of the prosecutions, but one that raises more questions than it answers.

First, credit should be given where due. There is an excellent discussion of why Nixon was named an unindicted co-conspirator, especially as it relates to the mechanics of making available to the House Judiciary Committee the materials compiled by the special prosecutor. There is also a compendium of the crimes that Richard M. Nixon may have committed and the federal statutes he may have violated while in office. The book provides a very useful summary of the arguments made by St. Clair and Jaworski before the Supreme Court on the constitutional issues involved in the president's claim of executive privilege, and does so in a way that would be useful to undergraduates studying the case. There is an appendix, "Status Report of Cases" which summarizes the various prosecutions and the penalties meted out, and for anyone interested there is a complete listing of the staff of the prosecutor's office.

There are several things wrong with this book. To begin with, the prose style reminds one of dentist chair music converted to words, pitched at the level of "My Most Unforgettable Moment." The clichés are bad enough, but it is almost impossible to believe that the wooden dialogue that Jaworski recollects was really what was said.

More important, the book will not satisfy anyone seeking an account of the structure, processes, and dynamics of the prosecutor's office. Descriptions are always of Jaworski dealing with one or two aides, and these dealings are presented in terms of the final alternatives which Jaworski must consider. There is only a superficial glimpse of the personalities involved, and of the possible courses of action (with the exception of the Kleindeinst prosecution and the decision not to indict

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Nixon, which are discussed at some length). It is interesting to note, however, that on a number of occasions Jaworski deals directly with Haig or other members of the White House staff, and either keeps his own counsel or confers with one or two top aides under a strict admonition of confidentiality. Clearly there was one chief for the prosecution, capable of dealing unilaterally with the most sensitive matters. How staffers reacted to this situation will only be revealed when their accounts are published.

The most serious flaw in the book is its failure to defend adequately the conduct of the prosecutions. First, Jaworski does not explain why no attempt was made to determine the source of funds which were used to finance some of the illegal activities. A "Mexican connection" was established for the laundering job, but the original source was never determined. Second, Jaworski defends the decision to drop several serious charges against Kleindeinst with the claim that he acted in good faith in protecting the president. Conveniently forgotten are the facts that the president placed pressure on him to drop a prosecution of an antitrust case, and that Kleindeinst participated in a cover up to avoid revealing the pressure. The attempt to exculpate Kleindeinst is unconvincing. At another point Jaworski slides over Haldeman's attempt to cut a deal in return for his testimony. By his own account the special prosecutor did nothing to encourage such a deal, even when it was suggested by Haldeman's attorney in the judge's chambers. Had Haldeman made a deal, his evidence might have been used to indict and convict Nixon of various crimes. But Jaworski seems never to have wanted to indict Nixon, and did not move to do so after the president's resignation—against the advice of everyone consulted on his staff. Jaworski's account of the pardon issued to Nixon denies any role of the special prosecutor in Ford's decision. But questions still remain. Did Ford know of the original cover up when he ran interference for the president before the Patman committee in the fall of 1972? Did Ford promise Nixon a pardon as part of the arrangement whereby Nixon resigned from office? Did Jaworski offer direct or indirect assurances that no indictment of the president would occur once he left office, and that no opposition would come from him if a pardon were granted? It may be Nixon's final legacy to us all that questions like these are asked, and that the memoirs of respected public servants cannot put to rest the suspicion that, in some ways, we are still being had.

RICHARD M. PIOUS
Barnard College, Columbia University

Setting National Priorities: The Next Ten Years *edited by Henry Owen and Charles L. Schultze. Washington, D. C., The Brookings Institution, 1976.—xvii, 618 pp. Cloth, $14.95; paper, $6.95.*

In each of the past six years the Brookings Institution has published a *Setting National Priorities* volume analyzing the budget proposals for the coming fiscal year. The Brookings team takes a far broader view of its task this year, addressing vir-

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tually all the major issues facing the nation and looking ahead over a ten-year period. Those inclined to view Brookings as a haven for a Democratic government-in-exile can well treat this volume as a possible blueprint for the new Carter administration's program.

Setting National Priorities consists of thirteen chapters, apart from the Introduction, each a considerable work in its own right. The Brookings team sees the problems it addresses as falling into two categories: foreign relations and domestic issues. As an economist, this reviewer was inclined to group the chapters somewhat differently.

First, there are questions of national security. The Brookings team considers the problem of avoiding nuclear war, addresses tactical and strategic issues in defense policy and grapples with the thorny problem of nuclear proliferation. These chapters are highly stimulating although the latter two chapters are at times rather technical. The conclusion that the United States has vital interests in Europe, Japan, and the Middle East but should show military restraint elsewhere is a sensible compromise between Vietnam-style intervention and a reversion to isolationism.

Second, three chapters focus on hard choices that must be made regarding quality-of-life issues: safety, income security maintenance, and energy versus environment. The Brookings team is very much in the postwar Democratic liberal tradition: basically market oriented (removing price controls on crude oil is recommended) but sympathetic to governmental efforts to "improve" the workings of the market (a tax is also suggested to offset the distributional effects of oil-price decontrol [p. 436]).

Third, four chapters address economic issues. Coeditor Schultze has written a rather boring chapter on federal spending in which he mainly provides an analysis of the federal accounts as they have turned out in the past and as they would be if extrapolated into the future, assuming unchanged tax laws and expenditure authorizations. Given his new position as chairman of the Council of Economic Advisors it is unfortunate that he did not write about inflation.

The chapter on inflation was written instead by George L. Perry, and is disquieting. Perry writes "Today's continuing inflation poses serious problems for stabilization policy" (p. 309), but never seriously considers that the inflation might be *caused* by stabilization policy. He takes for granted that a balance can be chosen between unemployment and inflation (p. 295), but never attempts to refute the theoretical proposition that in the long run the only trade-off is between unemployment and *accelerating* inflation. The inflation analysis itself is little more than sophisticated cost accounting, and the prescriptions derive from the cost accounts. To hold wage costs down, have an incomes policy. To obtain a lower wage norm, make tax concessions (p. 320). To reduce inflationary expectations and hence wage rises, cut sales taxes (p. 320). Do virtually anything, it seems, *except* control the supply of money (p. 309). These are the methods that have been used to fight inflation in Britain, where retail prices today stand 65 percent higher than they did three years ago. It is to be hoped that this view of inflation is not shared in the inner reaches of the Carter administration.

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Finally, there are chapters on the organization of the federal bureaucracy and on the relationship between Congress and the president. The latter chapter is both the shortest and the most fascinating in the book.

PAUL ATKINSON
University of Cambridge

Henry Wallace, Harry Truman, and the Cold War *by Richard J. Walton. New York, The Viking Press, 1976.—x, 388 pp. $12.95.*

Amid the post-Vietnam reassessment of such earlier critics of American diplomacy as Charles A. Lindbergh and Robert A. Taft, the resurrection of Henry A. Wallace was inevitable. Wallace, first as a dissenting member of Harry Truman's cabinet and then as the Progressive candidate for president in 1948, steadfastly urged peaceful coexistance with the Soviet Union. He attacked the Truman administration's atomic monopoly, the influence of the military-industrial complex, the continuation of conscription and heavy taxation, and the creation of military alliances and air bases to contain Russian expansion. Richard J. Walton, author of an excellent revisionist study of John F. Kennedy's foreign policies, has performed a labor of love in recounting Wallance's cold war dissent, for Walton himself had, as a young man, joined Wallace's Gideon's army of Progressives in 1948.

Walton's book rests on sound scholarship. He has tapped the huge collection of Wallace papers at the University of Iowa, interviewed Progressive veterans, utilized the Americans for Democratic Action (ADA) collection, and examined various papers at the Truman Library. In what might be called a moderately revisionist interpretation, Walton follows Thomas Paterson on the economic origins of the early cold war, Alonzo Hamby on the motives of Truman and the liberals, and Curtis Macdougall on Wallace's 1948 campaign. Perhaps the most original part of the book is Chapter 2, dealing with Wallace's tenure as secretary of commerce under Truman. Drawing heavily on Wallace's recently published diaries, Walton describes a Truman "exceedingly eager to agree with everything I said. He also seemed eager to make decisions of every kind with the greatest promptness. . . . It almost seemed as though he was eager to decide in advance of thinking" (p. 36). Thus does Walton explain Wallace's departure from the cabinet following his celebrated Madison Square Garden speech on September 12, 1946, as largely the result of Truman's eagerness to please both Wallace and Secretary of State Byrnes and his inability to understand that they advocated irreconcilable policies. In Walton's estimate, Truman's vaunted decisiveness covered up a simplistic mind that actually considered Wallace "a sabotage front for Uncle Joe Stalin" (p. 114). For Truman and the majority of his advisors, the cold war became a replay of World War II, with Stalin replacing Hitler as the insatiable aggressor. In such a context, Wallace's arguments suggesting a Soviet sphere of influence in Eastern Europe smacked of Munich-style appeasement, and Wallace's political supporters were considered fifth columnists.

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If there is a weakness to Walton's book, it is his unabashed bias in favor of Wallace. He admits there were warts, criticizing Wallace's coldness, his unwillingness to give attention to practical politics, the Progressives' failure to accept the "Vermont Resolution" in 1948 and thereby castigate Soviet foreign policy. Nonetheless, Wallace remains a hero—the prophet who predicted that "Harry Truman's foreign policy would lead to disaster at home and abroad. Henry Wallace was right" (p. 355). Walton rightly deplores the "red-baiting" of Wallace in 1948, but one should recall that Wallace, as FDR's running mate in 1940, had said: "Every Republican is not an appeaser. But you can be sure that every Nazi, every Hitlerite, and every appeaser is a Republican" (quoted in Robert A. Divine, *Foreign Policy and U. S. Presidential Elections, 1940–1948*, p. 59). Even though Walton does mention Wallace's personal foibles, there is nothing resembling the acid portrait in Lillian Hellman's recent memoir, *Scoundrel Time*. Nor does Walton's study probe why Wallace recanted after Korea and could actually tell Truman: "I'm glad you fired me when you did" (p. 351).

In sum, Walton has superbly analyzed the development of Wallace's cold war dissent between 1945 and 1948. For a full-dress portrait of the man, one must await the forthcoming biography by Theodore A. Wilson.

JOHN GARRY CLIFFORD
University of Connecticut

Social Program Implementation *edited by Walter Williams and Richard F. Elmore. New York, Academic Press, 1976.—xv, 299 pp. $14.50.*

In the past, social scientists have written a good deal about the initial stages of public policy making: the genesis of new ideas and the passage of congressional bills that contain those ideas. Much less attention has been given to the implementation of public policy—what happens after bills have been passed, funds committed, and agency goals set. But in recent years, scholarly attention has increasingly shifted to the study of policy implementation. And this volume brings together a number of insightful case studies that explore implementation problems in various program areas. Most of these essays have appeared previously, but presenting them together is useful for both scholarship and teaching purposes.

The Johnson administration's ambitious domestic programs have provided much grist for the mills of those who study implementation difficulties. In this volume, Edward Banfield's study of the model cities program shows clearly the obstacles facing comprehensive, coordinated action at the national level. And Martha Derthick's examination of a "new towns in-town" project in Washington, D. C., illustrates the limits of federal power even in what might appear to be a much easier undertaking.

Those who are critical of federal categorical service programs often call for reliance on transfer payment programs instead. But Beryl Radin's account of the implementation of the Supplemental Security Income program, which federalized

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adult assistance programs of aid to the aged, blind, and disabled, shows that cash transfer programs can have serious administrative difficulties of their own. In this case, for example, the Social Security Administration had a hard time responding to a different class of clients—those who had a legislative right to benefits but made no contribution to the program.

Another solution often suggested for problems in program execution is the movement toward "New Federalism" types of bloc grants to states and localities. But Jerome Murphy's study of Title V of the Elementary and Secondary Education Act, which provides discretionary funding to state education agencies, demonstrates convincingly that bureaucratic inertia is not a phenomenon found only at the federal level.

Even if programs do not often work out as initially envisaged, it might be hoped that we could learn from the experiences of past programs and make more informal choices in the future. To this end, there have been a number of attempts to design social programs as social experiments. This volume contains two interesting accounts of such efforts—Richard F. Elmore's essay on the planned variation experiment in the Follow Through program and Edward Gramlich and Patricia Koshel's study of educational performance contracting. Both essays bring out some of the conflicts that arise between the requirements of experimental design and the organizational and political incentives to which program actors respond.

In general, the essays in this volume are well written and sensitive to the complex organizational realities of program implementation in the American political system. The book would be even more valuable if its editors had done more to build on the case studies in bringing out general points about the implementation process. But this task is easier called for than done, and the essays collected here will serve as useful materials for those who wish to try.

JEFFREY L. PRESSMAN
Late of the Massachusetts Institute of Technology

The Moon and the Ghetto: An Essay on Public Policy Analysis *by Richard R. Nelson. New York, W. W. Norton & Company, 1977.—xii, 159 pp. $7.95.*

Experienced analysts of public policy are prone to write two kinds of books. In one, the analyst comes to terms with his experience by suggesting how the central institutions of government could be changed so that rational methods of approaching problems and their solutions would find a more hospitable home. In the other, the analyst accepts, more or less, the central institutions of American democracy and, rather than seeking to change them, seeks to change analysts—their focus of attention and their actions. Nelson's iconoclastic and occasionally frustrating essay belongs in the latter genre, and as such it is certainly worth reading and discussing.

Within the metaphorical framework of the moon-ghetto contradiction—an inappropriate choice of metaphor, since the focus of the discussion is not on success-

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ful versus unsuccessful policies but on policy analysis itself—Nelson identifies three "points of view" or "intellectual traditions" which involve "some rather strong preconceptions about the nature of problems and solutions, preconceptions that may turn out to be fruitful or unfruitful" (p. 15). Policy analysis is a useful interpretive mechanism, says Nelson, but one which is limiting or even misleading, depending on the intellectual tradition which influences it.

The first such tradition is that of "rational analysis"—or economics, operations research, and systems analysis. Nelson's discussion of rational analysis, while correct, is a bit shopworn. Thus he points out the emphasis on economic values and on "public policy decisions as 'investment' decisions" (p. 25), the "bias toward quantification" (p. 28), the difficulty of specifying objectives, the disruptive effects of analysis on political bargaining, and the poor organizational perspective according to which, somewhat foolishly, analysis is expected to help the client steer solely from the top.

The second tradition focuses on the choice and change of organizational structure. Here Nelson singles out sociology and political science for criticism. Perhaps "a sufficient reason for the lack of influence of the sociological tradition has been that on the one hand it is normatively weak, while on the other hand, it lacks a theoretical structure for generating predictions of the consequences of organizational changes, in the dimensions of most interest to policy makers" (p. 41). The linkages of organizational theory to policy choices are indeed almost nonexistent, but it is doubtful that sociologists will accept the judgment that their tradition is "normatively weak."

Nelson also accuses political scientists and scholars of public administration of lacking a "useful normative apparatus, and an ability to make persuasive predictions" (p. 45), as well as of being overly descriptive and of putting forth a "sophisticated cynicism." Even economists are criticized for overselling a narrow perspective and for their predictive model, which is not "anywhere near as good as advertised" (p. 51). One wonders why the organizational psychologists were left out of what is essentially a critique of the applicability of social science to the organizational component of policy analysis.

The third tradition is science and technology, or research and development (R&D). This discussion seems a little out of place here, but it is sensible for analysts and scholars to reduce their expectations as to what R&D can accomplish, given the nature of social policy problems and the difficulties inherent in organizing science and producing knowledge.

In short, policy analysts have been working with three intellectual traditions which leave much to be desired. What should we do? Besides "sharpening up of the separate perspectives" (p. 78), "we must overhaul and enrich the way we characterize and analyze organizational alternatives" (p. 79) and recognize the "open-ended evolutionary way in which policies and programs do and should unfold" (p. 79). We are urged to pay closer attention to the "institutional context and historical genesis . . . of social problems" (p. 152).

Nelson attempts to ground this advice to outlining the policy problems of extra-

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family day care, the supersonic transport, and the nuclear breeder reactor. He also sketches out in a few pages (pp. 136–139) a conceptual treatment of an evolutionary view of organizational structure. His discussion is much too skimpy, however, to tell whether what is being suggested represents a significant advance over the supply-and-demand thinking of economists or the open-system perspective of organizational theorists.

Nelson's slim essay does give good advice, but more is needed. How we should design and choose between organizational structures and institutional rules and how we should integrate historical and genetic perspectives into our thinking about public policy still remain serious and tough questions.

ARNOLD J. MELTSNER
University of California, Berkeley

Models in the Policy Process: Public Decision Making in the Computer Era *by Martin Greenberger, Matthew A. Crenson, and Brian L. Crissey. New York, Russell Sage Foundation, 1976.—xx, 355 pp. $15.00.*

This exceedingly useful volume, another product of the Russell Sage Foundation's long-standing concern with the relationship between the social sciences and public policy, is less valuable as a whole than for the sum of its parts. The authors—two applied mathematicians/computer scientists and a political scientist—set for themselves the ambitious task not only of describing the place of formal quantitative models in policy making, but also of prescribing methods for developing better linkages between modelers and political actors, with the explicit objective of increasing the usefulness of modeling activity.

The core of the book is a description of many of the most influential quantitative models: land-use and housing stock models; Jay Forrester's Systems Dynamics and its famous offspring, the Club of Rome's *Limits to Growth* study; the municipal service models created by the New York City Rand Institute; and, most notably econometric forecasting models. These models and their underlying methodologies are presented with a crystalline clarity that makes this the best primer this mathematical primitive has ever encountered. Surrounding the descriptive material is a first-rate, if extremely condensed, intellectual history of much of modern quantitative social science.

For anyone interested, whether for teaching purposes or just an improved comprehension of the newspapers, in what *Limits to Growth* or the recurrent debates among macroeconomic forecasters are really all about, this book is therefore indispensable. Its prescriptive contents are less satisfying. The authors certainly cannot be accused of political insensitivity; their accounts of the politics of model building and model selling manage to combine sound political reporting with fascinating academic gossip. But while their judgments of models and modelers are balanced and sound, their discussion of the political context within which social science

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is utilized is inadequately systematic and excessively concerned with formal structure.

Given the enormous volume of literature, some of it actually worthwhile, on the relationship between social science and policy—a literature with which Greenberger, Crenson, and Crissey seem adequately familiar—their prescriptions for the use of models are disappointing. But that disappointment detracts only marginally from what is otherwise an important contribution to communicating to the general public some of the more significant mysteries of contemporary social science.

BRUCE C. VLADECK
Columbia University

Poliscide *by Theodore J. Lowi, Benjamin Ginsberg et al. New York, Macmillan Publishing Company, 1976.—x, 306 pp. $4.95.*

On December 15, 1966, the Atomic Energy Commission announced that it had selected a site near Chicago, Illinois, as the home of what was to be the world's largest accelerator. The atom-smasher was a much-sought-after prize, and the news of the AEC's choice pleased Illinois citizens—including the residents of Weston, a recently incorporated village of 71 farms and 100 additional houses, which was included in the site acreage.

By late 1968, 170 of Weston's 171 households had been acquired by the AEC, and their residents had moved elsewhere. *Poliscide* asks how the nascent community could have been put to death with so little opposition. And the answer seems to be that Weston died because the surrounding DuPage County had never wanted it, because the accelerator gave the county a decisive weapon against it, and because no one else involved in the project had any interest in weighing into the battle between county and village.

In the late 1950s, when the Weston story began, DuPage County was determined to maintain its affluent suburban character by regulating the nature of its industry and housing. But it was in danger of succumbing to hostile forces. Despite DuPage's opposition, the village of Weston was incorporated within it in 1963, with inexpensive homes and with a history—including blatant subterfuge by fly-by-night developers, federal takeover, and even Mafia financing—of the kind that gives exclusionary zoning a good name. By the mid-1960s, county and village were locked in stalemate: the village could not expand significantly, but the county could not kill it.

The county was saved by the AEC's decision to locate the new accelerator at Weston—a decision heavily influenced by the fact that midwestern scientists, the state of Illinois, DuPage, and the majority of Westonites themselves wanted the facility. Soon after the announcement, the AEC decided that it wanted to build on the actual land under Weston's homes, and that it could save money by acquiring the structures themselves. The state of Illinois, handling the negotiations with Weston, obliged by making it difficult for the villagers to exercise their right to buy

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back their homes. And even had the villagers managed to retain the houses, their DuPage neighbors would not let them annex new land to move to *en bloc*. So Westonites moved away individually, though sometimes not very far; the village as a corporate entity was dead.

The authors consider this outcome a sign of nothing less dismal than Niebuhrian "moral man and immoral society" (p. vi). They say that to prevent such results, the present divided decision making of the federal system must be radically revised to subordinate scientists and local elites alike to a coherent public interest incorporating a broad spectrum of social values. And this revision is to be accomplished "by rules . . . that attempt to plan for some of the social consequences of each project" (p. 293).

These verdicts would be less puzzling if readers were given some more information—if the authors were more successful at explaining, for instance, why public opinion in Weston itself was on balance favorable to the accelerator, or if we were assured that the social costs associated with other technologically feasible sites were unambiguously lower than Weston's. We might also have more confidence in the book's judgments if we were spared such egregious signs of partisanship as, " . . . one is led seriously to ask whether negative planning [i.e., keeping out undesirables] is a luxury only available to the bad guys" (p. 192). And, finally, we might be less skeptical of the call for more centralized rule making were there not so much evidence—in the Weston story itself, but also in this country's experience with policies from poverty programs to the new wave of business regulation—that neither centralization nor more rules is a guarantor of good policy outcomes. One may, in other words, regret the death of Weston, Illinois, without feeling able to embrace the full freight of implications that *Poliscide*'s authors claim for this experience.

SUZANNE WEAVER
Yale University

Neighborhood Organization and Interest-Group Processes *by David J. O'Brien. Princeton, N. J., Princeton University Press, 1976.—xii, 263 pp. $13.50.*

"In the mid-1960s when I first began to think about neighborhood organization," David O'Brien writes, "many of us were enveloped in a mood of confidence about the ability of the American people to make their institutions more responsive. . . . Today that optimism has been replaced with confusion and doubt." O'Brien then asserts that "the source of our current malaise is . . . our failure to understand, . . . an inability to formulate meaningful questions" (p. ix).

To provide the proper understanding, O'Brien comprehensively reviews a vast literature on community organization, social protest, community action, organizational behavior, and urban politics. His discussion ranges widely, touching on the work of Goodman, Jacobs, Nisbet, Reich, Alinsky, Galbraith, and Kotler. And through his review of secondary sources, he considers the experiences of European immigrants, labor during the New Deal, new-left organizations, Alinsky-founded

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community groups, and—at considerable length—the community action program. His treatment of these topics is always sensible and judicious; indeed, O'Brien seems to have put into one package the retrospective conventional wisdom on many writers and events that dominated community politics in the 1960s. And as a result, the book can serve as a valuable, if somewhat elliptical, introduction to the community organization literature.

But the book has its own distinctive point of view—that of public choice theory. In a most useful adaptation of Mancur Olson's ideas to the community organization context, O'Brien argues that the major obstacle to organizing the poor has not been the ineptitude, alienation, or subservience of the poor but the difficulties that community groups have in either providing selective incentives to neighborhood residents or coercing them to participate in group efforts. In this regard, neighborhood groups suffer even more than do the groups Olson considered, because they lack legitimacy either in the community or in the larger society. And without legitimacy, coercion is impossible and selective incentives are difficult to procure.

According to O'Brien, most organizational strategies have mistakenly addressed secondary issues rather than concentrating on establishing group legitimacy and maintenance resources. New-left groups tried to create participatory mechanisms for honest self-expression; Alinsky-style groups wanted immediate victories on relatively insignificant substantive issues; and community action agencies were founded on consensual rather than institutionalized-conflict assumptions. Successful neighborhood organization will not be achieved, however, until groups win for themselves a legitimate position within the societal institutions that adjudicate conflict.

The analysis is suggestive, but as with most prescriptions for neighborhood government, the author more clearly defines the obstacles than the means for achieving it. Even though he sees "our failure to understand" as the nub of the problem, he never explains how groups can achieve the legitimacy they need. Instead, his conclusions wander off to such topics as metropolitan federalism, neighborhood corporations, and voucher plans. But perhaps it is unfair to criticize an otherwise sound attempt at scholarly advocacy for failing to achieve what may be utopian.

PAUL E. PETERSON
University of Chicago

Patterns of Interracial Politics: Conflict and Cooperation in the City *by Peter K. Eisinger. New York, Academic Press, 1976.—xvii, 202 pp. $11.00.*

Patterns of Interracial Politics is an analysis of the attitudes of a sample of black and a sample of white citizens of Milwaukee, Wisconsin, conducted in 1970. The book attempts to identify patterns of "mass" belief on the theory that they provide "constraints" or "contexts" within which "elites" must operate. Three patterns are identified: (1) neither blacks nor whites care much about "community"; (2) they

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have somewhat differing priorities in regard to concrete issues but the consensus and satisfaction with the police and the schools is actually surprisingly high; (3) whites are more active in conventional patterns of political participation, blacks more active in protest politics. Eisinger then struggles to tease some theoretical significance out of the survey data—there is a "typology" of interracial political relationships, and an inventory of "propositions" about the meaning of mass attitudes for elites. A central conclusion is that because of the fragmented structure of mass opinion in Milwaukee's black community, black leaders are not likely to be very strong and, therefore, white hegemony is likely to persist.

Does this survey and analysis increase our understanding of Milwaukee, interracial politics, or theories of urban politics? The profile of the city is too thin to augment or change one's view of the city from Schmandt, Golbach, and Vogel (*Milwaukee: A Contemporary Urban Profile*, 1971). The survey is less comprehensive than that reported for Detroit in 1971 by Aberbach and Walker (*Race in the City*, 1973). The theoretical analysis is most interesting when it addresses the issue of whether the masses care who controls the city. Greenstone and Peterson (*Race and Authority in Urban Politics*, 1973) have attempted to explain black political behavior from the elite viewpoint as a product of efforts to establish authoritative control over positions rather than to induce other elected officials to act. Eisinger's notions imply that the masses care little about such niceties. (Only 23 percent of black respondents, for example, had even heard of the local organization which conducted the most militant fight for black control over federal programs.) Perhaps we ought to take more heed of this mass apathy about control. There are lots of people today struggling for control—unions, minority leaders, reformers, candidates—but the plain fact is that none have the strength to reorganize and deliver services. The Milwaukee "masses" seem to know this all too well.

FRANCINE F. RABINOVITZ
University of Southern California

Black Elected Officials: A Study of Black Americans Holding Governmental Office *by James E. Conyers and Walter L. Wallace. New York, Russell Sage Foundation, 1976.—xi, 190 pp. $8.95.*

James E. Conyers and Walter L. Wallace end their book by observing: "the rapid achievement of political, economic, and social equity between the Black and the White communities and of equality between Black and White individuals remains an indispensable defense of American democracy" (p. 159). That statement, with its obvious superficiality (and obscurity) is, nevertheless, one of the more likely observations made by the authors in what is generally an uninspired volume. Prospective readers should not be misled by the title; the Conyers-Wallace book is actually a study of the *attitudes* of a sample of 799 locally elected black officials that answered a mailed questionnaire in 1971. Haphazardly tagged on at the end is a chapter of quotations from personal interviews with thirty-four black officials,

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including six United States representatives. The authors are themselves careful to point out the modest character of their undertaking, terming it exploratory. However, the lack of theory and their seeming unawareness of a large political science and black politics literature germane to their analysis renders even the exploratory relevance of this study questionable. Gathering and manipulating data is not an end in itself and without paradigm-generated puzzles and/or a theoretical framework it is a fruitless inductive enterprise.

In the absence of theory and/or grounding in the relevant literature, Conyers and Wallace produce statements that are either potentially "meaty" but lack empirical verification or empirically derived but trivial. For example, they tell us: "There can be little question that the Black community, during the past ten years, has come to wield greater direct power than ever before in the political and governmental life of the United States" (p. 159). Their attitudinal data do not and could not be the basis for that opinion. More importantly, it slides past a number of questions: What is meant by direct power? Should the black community be analyzed as a monolith? If so, for what purposes? Should the white community also be analyzed as a monolith? Is this alleged "direct power" the result of the civil rights movement, black militancy, or black elected officials? Some black political scientists who have looked thoughtfully at black politics during the last decade have concluded that blacks may have won the trappings of power rather than real power. In contrast to the above "meaty" but dangling throwaway line is a "research finding" that, "Older Black officials are shown to have been more integrationist, more conservative and less accepting of direct action . . ." (p. 60). Although amply supported by their data, it is hardly a startling or penetrating observation. So too their "major" conclusion that: "our study has emphasized how easily a Black official could tend to be a racial 'radical' without being a socioeconomic 'radical' and how easily he or she could be either without being a gender 'radical' " (p. 155) is neither unexpected nor earthshaking.

The significant aspects come from the inclusion of quotations of black elected officials and from the seventeen-page chapter in which Conyers and Wallace "give Black elected officials a chance to speak more freely for themselves . . ." (p. 137). Their respondents are more theoretical and profound than the authors. One observes: "they [some Negro elected officials] are elected by Black votes but are run by the White power structure. So, therefore, when anyone makes an attempt to effectively move to abolish a policy or a law that was created to hurt Black people, they are often the first persons to come up with another idea which will mediate it or effectively kill the amendment . . ." (p. 60). That statement undermines the simplistic view of a monolithic, unified black community and raises serious questions about how and for whom black elected officials use their official status (the potential for sharp contrast between what they do and what they say they do is obvious).

Obviously a lot of money and time went into this research but it is difficult to know what audience would benefit from the results. Scholars and students of black politics would find it dated (1971 is eons away when attitudes are being examined

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that so obviously are shaped by current events), theoretically effete, and uninformed; political scientists specializing in attitudinal research would find minimal methodological innovations; even the informed layman is not likely to plod through the stilted, often pompous, writing style.

MARGUERITE ROSS BARNETT
Howard University

Black Ballots: Voting Rights in the South, 1944–1969 *by Steven F. Lawson. New York, Columbia University Press, 1976.—xii, 474 pp. Cloth, $20.00; paper, $6.95.*

This well-written volume describes the rise and fall of the black disfranchisement in the South. While the subtitle indicates that the book covers a quarter century beginning with 1944, the early chapters actually provide a more than adequate treatment of the steps by which blacks were systematically excluded from voting almost half a century earlier. The bulk of this work traces the slow, but ultimately successful, series of judicial, legislative, and executive actions which have largely removed the barriers of racial discrimination from the polling place. More specifically, the author traces the line of cases attacking the white primary and the events surrounding the passage and implementation of the civil rights legislation enacted between 1957 and 1965.

Political scientists will be favorably impressed by the sensitivity to political constraints displayed by the historian author. Lawson is careful to offer a political explanation for the hesitancy shown at times by Democratic presidents Roosevelt, Truman, and Kennedy to press for fuller federal voter protection laws. He notes the unwillingness of these chief executives to antagonize powerful southerners in Congress, thereby endangering other programs, in an effort to get voting legislation passed.

Lawson is equally sensitive to the political pressures which accounted for the passage of four acts affecting black voting rights. He documents the influence of a growing black electorate in the North. Blacks in the North "could use their ballots as a balance of power in close congressional elections, or they might swing their states with large numbers of electoral votes into the winning presidential column" (p. 346). Another factor—and it is one largely ignored by others—emphasized by the author in explaining why after eighty years of inaction Congress finally came to the rescue of disfranchised southern blacks is the United States Commission on Civil Rights. As Lawson shows, the commission served as a catalyst for social change by being an independent critic of both local obstinancy and federal hesitancy. The periodic recommendations made by the commission for new or remedial legislation often set the agenda for future congressional debates.

Lawson describes the congressional debates preceding passage of each of the civil rights acts. He also shows the influence of external forces, such as violence perpetrated against civil rights activists, public opinion, and lobbyists.

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An added feature of *Black Ballots* is that it is not limited to just the formulation of new legal standards concerning the suffrage. Instead, it goes on to consider the implementation of the statutes. The obstacles to the 1957 and 1960 legislation and the defects in these early acts are driven home with a number of moving examples. Both the strengths and weaknesses evident in the responses of the executive and legislative branches are noted.

The author emphasizes the significance of the right to vote by repeatedly quoting black activists as well as sympathetic white politicians who saw access to the ballot box as the key which would open the door to equal treatment in other spheres. Lawson, however, is too good an analyst to accept these expectations without qualification. After reporting the increases in black registration and office holding and more briefly noting some symbolic and material gains associated with black political activity, Lawson concludes that "On balance, a 'new day' had dawned for many southern Negroes; but liberals, black and white together, had expected too much of a payoff from enfranchisements" (p. 352).

CHARLES S. BULLOCK III
University of Houston

The Black Family in Slavery and Freedom, 1750–1925 *by Herbert G. Gutman. New York, Pantheon Books, 1976.—xxviii, 664 pp. $15.95.*

Increasing numbers of scholars, both black and white, are turning their attention to a most remarkable phenomenon—the evolutionary development of black family life in America. Even when scholars and researchers disagree on the causes, consequences, and manifestations of black family life, there is an emerging consensus among serious and open-minded scholars that the black family did indeed survive slavery. Moreover, researchers are beginning to assess the importance of the African background of the Afro-American family. We are now clear that even though the most instrumental aspects of African culture were virtually wiped out by the slave experience, the more expressive aspects of that culture remained to help the African bondsmen fashion new lives for themselves and their descendants in America.

But the Afro-American family is not the African family. It is a new creation—a coming together of four major streams of world culture. The Afro-American family is a product not only of cultural influences from Africa, but of those that emanated from Europe, the culture of their captors, and the culture of America itself which evolved in ways different from Europe. Moreover, the basic conditions of life for blacks in America which were different from the conditions of life for free whites played an enormous role in fashioning both the structure and the function of black family life in America.

Herbert Gutman's work, *The Black Family in Slavery and Freedom*, shows that between 1750 and 1925, blacks in America were able to establish and maintain stable patterns of family life. The book is long and cannot be characterized as easy reading. Readers who are willing to invest the necessary time and effort, how-

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ever, will be rewarded by the sheer knowledge and information contained in this book.

Part One is composed of eight chapters under the heading, "The Birthpangs of a World," which focus on the early experience of slavery in America. The positive approach Gutman takes to his subject is expressed in his opening sentence: "This is a book about ordinary black men, women, and children mostly before the general emancipation but after that time, too—a study of enslaved Afro-Americans, their children, and their grandchildren, how they adapted to enslavement by developing distinctive domestic arrangements and kin networks that nurtured a new Afro-American culture, and how these, in turn, formed the social basis of developing Afro-American communities, which prepared slaves to deal with legal freedom" (p. 3).

This is admittedly a large order, but since Gutman has been at work on this book for nearly a decade and is very careful to let his data generally speak for themselves, he has produced an outstanding study. His insight into Afro-American families is profound. Citing what he terms "adaptive kin arrangements," Gutman shows that blacks early clung to both the ideas and the reality of family life even though they were not always permitted to settle down with one partner till death.

Consider the case of Samuel Johnston, born as an eighteenth-century Virginia slave. "Upon his manumission in 1810, Johnston was allowed to remain in Warrentown, and in the next five years he raised sufficient cash to purchase his wife Patty, his daughter Lucy, and his son Samuel. He petitioned the Virginia legislature for their freedom in 1815. For reasons unknown to us, the legislature refused to manumit his children" (p. 35). But again in eight different years between 1820 and 1837, Johnston repeatedly petitioned the legislature to free his wife and children.

Beyond the fact of family relations involved, this shows the supreme value which blacks placed on family life and the extraordinary efforts that often went into attempts to maintain their families. Gutman concludes, unlike other students of the question, that slavery did not succeed in destroying family life among blacks and that the family not only persevered during slavery, but survived as the basis of a new culture on the American continent.

He shows extensive family history from a cross-range of plantations, often stretching over several generations. Thus, the slaves were able to establish families and large kinship networks, develop their own rules governing marriage, adapt new patterns of naming their children, and maintain communities in wide and different parts of the slave-owning South. What Gutman does not deal with quite definitively is the question: Why did blacks maintain patterns of family life? Was it because the slave owners were finding it economically feasible to encourage families, as is claimed by Fogel and Engerman in *Time on the Cross;* or was it because slave owners were humanitarians at heart, as claimed by Eugene Genovese in *Roll, Jordan, Roll;* or did the slaves maintain their families and develop a new culture despite the oppressive conditions of slavery and their masters? Gutman comes closest to the latter view, but does not develop it fully.

Notwithstanding this, however, Gutman has the data in this book which both

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sociologists and historians will be turning to in years to come in order to understand the resiliency and adaptability of the honor and spirit so well exemplified in the evolution of black families in this country.

ANDREW BILLINGSLEY
Morgan State University

Television as a Cultural Force *edited by Richard Adler and Douglass Cater. New York, Praeger Publishers (Special Studies), 1976.—191 pp. $17.50.*

The Manipulators: America in the Media Age *by Robert Sobel. Garden City, N. Y., Anchor Press/Doubleday, 1976.—xxii, 458 pp. $9.95.*

Here are two more offerings on the altar of media studies. One is over 400 pages by a single author; the other crams the work of nine authors into less than 200 pages. One is turgid; the other sprightly. Both promise more than they deliver.

In *The Manipulators*, Robert Sobel offers a potted history of newspapers, radio, motion pictures, television, and higher education in America. In the process, he mentions innumerable topics including the Hollywood Ten, blacklisting, censorship, internal propaganda during World Wars I and II, the role of the professoriat in such propaganda, the impact of veterans on colleges and universities, and media as escapism. Franklin, Pulitzer, Creel, Wilkie, Murrow—a parade of personalities passes through the book.

It is doubtless useful to have so much media history in one place, but one misses themes, ideas, the point of all the detail and data pillaged so assiduously from secondary sources. Some issues are mentioned. There is the rise of an audience that "has come to dominate" creators and disseminators "and to a large extent force them to its will" (p. xiii). There is also the emergence and now domination of their professions by mass intellectuals, performing artists and professors who can address wider audiences than ever envisaged. This is referred to as "the single most important cultural and political force of our times, and pervades all aspects of our lives" (p. xx). We are told that "politicians have been obliged to become performers too, to conform with the requirements of these methods of transmitting ideas and entertainment" (p. xxi). Then there is the "merger of politics and show business" (p. 342), the intertwining of "reality and fiction" (p. 343) that these media, especially television, have effectuated. Sobel predicts, "that we may . . . have a plebiscitary presidency" (p. 389). "The future of American politics," he concludes, "appears to be in the hands of those individuals capable of swaying the public's emotions, playing upon love and fear rather than intellect and even self-interest" (p. 390).

Some of these thoughts are banal, others curious, none original. Yet, they are never logically connected or developed; they appear, rather, as aperçus in the first and final chapters and scattered among the descriptive material in between. The connection of higher education to the other information-entertainment industries is unclear and universities disappear in the final pages where most of the book's thoughts appear. Politics is interjected at the end as a primary focus, although

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viewed throughout most of the book only in relation to the development of the industries. Worse, despite the book's title, we never really learn who manipulates whom, how, why, and with what effects.

Television as a Cultural Force is another collection from the Aspen Institute Program on Communications and Society. It contains nine varyingly thoughtful, insightful, and provocative essays which show that the content of television programs is more complex than most observers realize—or, at least, that it can be made to appear more complex. It is the Institute's best volume to date.

In the book's most original essay, Peter Wood offers an interpretation of television as dream focusing on the similarities of their forms and effects. He suggests that "TV is a vivid projection of our collective subconscious" (p. 25) and concludes by asking whether "the interpretation of TV as dream [could] ever have any collective therapeutic value" (p. 34). Similarly stirring is David Thorburn's analysis of television as melodrama. He almost persuades me of the validity of his assertion that "recurring features of melodrama can be perceived as the *enabling conditions* for an encounter with forbidden or deeply disturbing materials . . . (p. 80).

Sharon Sperry applies narrative theory to television news—viewing it as the telling of a story. There is much in her argument and analysis with which to disagree. For example, it may not be that television news people impose a coherent narrative on disparate events but that they are attracted to events which have a clear narrative (and dramatic structure). Nonetheless, her approach is felicitous and she applies it imaginatively.

The remaining essays are less distinctive. Paula Fass analyzes the "Mary Tyler Moore" and "M*A*S*H" shows in terms of the underlying American myths and symbols they sustain. She suggests that television programs can "provide a focus for studying the slow process by which common images evolve within a recognizable context" (p. 56). Kevin Ryan approaches the influence of television on public morality by applying Kohlberg's cognitive-development theory to an episode of "Kung Fu." The application is quite primitive, the idea remains intriguing. Kenneth Pierce nicely evokes "All in the Family" and speculates about its appeal. He argues that the show's "characters are distinguished by their relation to 'the news,' and that the episodes portray the disruption of family life by social issues" (p. 70). Robert Alley pedestrianly surveys the history of medical dramas on television. Perhaps constrained by space limitations, David Littlejohn struggles to analyze his abundant materials on television criticism. He concludes with a series of recommedations the most deserving of which seem to me to be the hardest to achieve. The book ends with a reading list compiled and usefully annotated by Christopher Sterling.

Despite its merits, the book is dilettantish, using television as a text for literary-symbolic glosses. The methodology is primitive at best; none of the authors show any familiarity with the rudiments of content analysis. Nor do they use relevant communications research. The neglect of "uses and gratifications" and "effects" research is particularly deplorable because many of the contributors speculate wantonly about the reasons people watch, and the effect of, television. Granted, it is

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never easy to inspire an original set of essays. The book would be better had the editors more vigorously challenged their contributors to develop and explore the implications of their approaches.

DAVID L. PALETZ
Duke University

What Women Want: The Ideas of the Movement *by Gayle Graham Yates. Cambridge, Mass., Harvard University Press, 1975.—xi, 230 pp. $10.00.*

Few readers will truly discover "What Women Want" in this book by Gayle Graham Yates. More a review of major popular books associated with the woman's movement over the past decade than a thoughtful review of the ideas and their impact, Yates has written an overlong annotated bibliography, interspersed with her own value judgments.

In an attempt to be scholarly, and perhaps bound by the requirements of her Ph.D. committee (this book was a dissertation), Yates constructs a typology of woman's movement ideology. But it is empty—an untheoretical framework. It is also often incorrect. Can Yates really make a distinction between a "feminist perspective" and a "woman's liberationist perspective"? Does any person or group accept this boundary distinction? What is this "women's liberationist paradigm" which provides a model of "women-over-against-men or . . . separate-from-men" (p. 19)? Certainly some groups, as she states, have advocated separatism, but they have been few and far between, though given considerable media attention.

But this is presumably not a media book but one aimed at informing the scholar and the popular reader interested in "women's studies." But none of these readers can tell from this book how the ideas of the woman's movement evolved, nor the relationship between ideas and movement activity. The ideas of groups such as the Redstockings, or SCUM, are presented with equal weighting to those of such national organizations as NOW or the National Women's Political Caucus.

It is also a pity that Yates did not include materials on the movement from historians like William Chafe or social scientists (Alice Rossi is the exception). Perhaps she would have been aided or oriented to assessment rather than reporting.

Although this book was written by a well-intentioned spokesperson for the feminist cause, it is enough to turn off even the most committed reader. It is repetitious and boring. Not only have the editors of Harvard University Press failed Yates in not tightening the writing and cutting redundencies, but they have also permitted ridiculous inaccuracies such as the consistent misspelling of Erik Erikson's name as Eric Ericson.

CYNTHIA FUCHS EPSTEIN
Queens College of the City University of New York

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Explorations in General Theory: Essays in Honor of Talcott Parsons *edited by Jan J. Loubser, Rainer C. Baum, Andrew Effrat, and Victor Meyer Lidz. 2 vols. New York, The Free Press, 1976.—xvi, xvi, 909 pp. $60.00.*

Talcott Parsons has been one of the great figures in modern social science for more than forty years. Though it may not show in the citation counts, his direct and indirect influence on social scientists in this period has probably been greater than that of any other person. The distinction of his generations of students and of his succession of collaborators, young and old, and the continued productivity and influence in turn of these students and collaborators, are measures of his enormous influence, past and present, in a variety of social science disciplines. The admiration, respect, and affection of his students for him has been frequently and widely expressed.

It is inevitable that such a figure should have not only the usual scientific critiques—he has had these in plenty, sometimes telling, sometimes merely prejudiced and ill-informed, but also testimonies to his originality and his importance. There has already been one *festschrift:* Bernard Barber and Alex Inkeles, eds., *Stability and Social Change* (Boston, 1971). The contributors to that volume were from the older, earlier generations of Parsons' students and colleagues—Neil Smelser, Robin Williams, Edward Shils, Albert Cohen, Clifford Geertz, and Robert Bellah. Now comes another *festschrift,* this one primarily from younger, more recent generations, for Parsons has managed to have serious and productive students for his whole scientific career. This is a mighty production—two volumes containing more than 900 double-columned pages of smallish print. There are thirty-eight contributors, some with multiple contributions. They come from many countries: the United States, Canada, Germany, Israel, France, Japan, England, and South Africa. Their disciplinary and subspecialty affiliations are about as diverse as their national origins.

In a brief review it is impossible to describe this diversity and richness in detail. But it is perhaps useful to indicate that the authors, as might be expected, are following up on many of the problems and themes opened up or developed by Parsons: the Hobbesian problem of the order and disorder of society; the dilemma of stability and change in social systems; the methodological indispensability of the concept of system; the structure and processes of formal organizations; the nature of professionalism; the processes of socialization, or how people are made human; and many more. The contributions not only expound, develop, refine, and justify the Parsonian theories, but they are also, as necessary, constructively critical. Those who know Parsons' theories best are often his best critics.

This is not a set of books for everyone, but it is indispensable for anyone seriously concerned with the making of social theory. It is one of the great achievements of modern social science that we have developed a sophistication and systematization in social theory that we did not have forty years ago. A large part of the credit for

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this belongs to Parsons and his students and collaborators. The achievement can profitably be studied in these welcome volumes.

BERNARD BARBER
Barnard College, Columbia University

Ideology and Utopia in the United States 1956–1976 *by Irving Louis Horowitz. New York, Oxford University Press, 1977.—xv, 464 pp. Cloth, $17.95; paper, $5.95.*

Professor Horowitz is not only among the most prolific of American sociologists, but his writings also cover an impressive range of subject matters. He has written extensively on, to mention just a few areas, Latin America, the arms race, science policy, the sociology of conflict, and international relations. This breadth of concern has led, however, to a good deal of superficiality. His best work provides novel perspectives and suggestive ideas, yet many of his writings do not go much beyond high-level journalism. Deep down he is often quite shallow.

The present collection of essays, most of which were previously published elsewhere, were written over two decades, and exhibit both Horowitz's strength and weakness. An essay on multinational corporations, for example, breaks new ground by focusing on the many intricate ways in which these corporations have managed in recent years to penetrate the Eastern block so that they now constitute a major bridge between state socialist and capitalist societies. Such ties between formally opposed economic systems, Horowitz argues, may in the future lead to a common front in economic affairs which might prove detrimental to the Third World countries.

In a similarly thought-provoking essay on "Race, Class and the New Ethnicity," Horowitz casts a skeptical eye on the current revival of concern with ethnicity among many sociologists and social commentators, and on the political uses of ethnicity which threaten to obscure the more basic realities of class and power. Comparing the new ethnics with the blacks and Jews, Horowitz shrewdly remarks that the former "represent groups that would like the future payoffs, but not the historical penalties, of becoming a group apart" (p. 60). Horowitz is suspicious of the resurgence of ethnicity which he sees as "a surrogate concept, an expression of disintegration and deterioration. . . . Like other notions of particularistic nature, its importance derives more from whom it excludes than whom it includes" (p. 73). There are a number of other fine essays here—on the unholy alliance of certain social scientists with military policy makers, on project camelot, on the politics of détente and disarmament—that are very worth reading.

But these excellent essays are unfortunately intermingled with others that were lightweight when they were written and appear even less appealing when reprinted here in unrevised form. What is one to make, for example, of two related essays on American conservatism and American liberalism in which conservatism is defined in terms of "the minimization of state authority and the maximization of

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individual opportunity and autonomy" (p. 136), while traditional liberalism is said to "always involve the assumption that government governs best when it governs least" ((pp. 170–191). The essays included in the section on presidential politics dealing with John Kennedy, Johnson, Nixon, and Watergate, and J. Edgar Hoover may have been good journalism when they appeared at the time but they seem dated and superficial when republished at a later date. The same holds true for an essay discussing the McNamara era and the gaming approach to world strategy that was then in the ascendancy. Matters are not helped either by Horowitz's sloppy use of language. Writing about "traditions [that] are not easily resolved" (p. 165), or about dislodging "the cold war from its gray eminence" (p. 330), Horowitz only comfirms the reader's impression that he writes too hastily and too much.

When all is said and done, this volume still provides enough fresh commentary and analysis to make it worth attentive perusal.

LEWIS A. COSER
State University of New York at Stony Brook

No Easy Choice: Political Participation in Developing Countries *by Samuel P. Hungtinton and Joan M. Nelson. Cambridge, Mass., Harvard University Press, 1976.—x, 202 pp. $10.50.*

However intangible the consequences for the Third World of the congressional decision to include "political participation" as an end and condition of United States assistance, the legislation known as "Title IX" brought the subject to the forefront of social scientific concern in American universities. *No Easy Choice* presents the results of a four-year research program funded by the Agency for International Development at the Center for International Affairs of Harvard University. Originally written as a report to the sponsoring agency, the work is also addressed to the social scientific community. It should therefore be reviewed both as a political document and as a scholarly one.

Political participation figured prominently as one term of Huntington's decade-old equation: If participation outpaces institutionalization, decay rather than development will result. Whatever the validity of his social scientific assertions, Huntington clearly suggested that contemporary developing countries were typically participation-rich and institutionalization-poor and that the problem, from the point of view of order-minded officials inside and outside these societies, was how to promote institutionalization. It is on this problem that Huntington focused his own attention. That an agency legally obligated to search for ways of promoting participation should then turn to Huntington for assistance is therefore an interesting political event that is, unfortunately, not discussed in the present book. Whether or not the administrators of Title IX anticipated it, the result of the research they sponsored is advice on how to contain participation rather than on how to bring it about.

The scientific message of *No Easy Choice* is related to the book's character as a

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political document. Critical of what they label the "liberal model of development," the authors present an approach to the political analysis of contemporary developing countries that abandons the search for historical and social scientific understanding on behalf of the pursuit of effective political results. From the latter point of view, what matters is the behavior of elites. Huntington and Nelson conclude that although socioeconomic modernization does in the long run foster "more extensive and more diversified political participation, a cyclical pattern is also likely to exist in the way actors resort to political participation as a means to achieve their goals" (p. 166). Individuals are not moved to participate in direct response to socioeconomic change; they are likely to do so only when "the group context" moves them, and the character of this context in turn owes a great deal to the orientation of elites. "Because of this 'cascading effect,' elite choices and attitudes toward participation and other goals are the most powerful determinants of the extent and nature of political participation in a society at any given time . . ." (p. 169).

This conclusion rests on a four-cell model of development applicable to "nations that have broken out of the low-level steady-state, but have not experienced a successful revolution" (p. 21), in which elites have two choices in each of two successive phases. In Phase One, the alternatives are a "bourgeois" or an "autocratic" model. The first, which grants political participation to the urban middle class alone, brings about economic growth but also growing socioeconomic inequality; the second, which denies participation to the middle class as well as to the lower classes, brings about distributional equality and hence support from the lower classes. Sooner or later, however, each model is faced with the problem that triggers off Phase Two: "Whether and how to provide for lower class political participation" (p. 23). The possibilities are now the "technocratic" model, with low participation, increasing inequality, but high investment (including foreign) and economic growth, a happy state jeopardized only by a looming "participation explosion"; or a "populist" model, egalitarian but stagnant, jeopardized by an overabundance of lower-class demands which cause a "participation implosion." The first leads to governmental repression, the second to civil strife.

Whatever the inadequacies of the "liberal model of development" or of more recent alternatives to it, this framework does little to provide the academic community with a better theoretical mousetrap. The four "models" are too gross to fit the variegated universe under consideration; and it is questionable whether there would be intersubjective agreement on the allocation of countries into each category. It is not clear whether the typology is based on the intent of rulers or on the objective outcomes of their choices, nor does the framework account for the determinants of elite choices. It is by no means self-evident that the "bourgeois" model in fact leads to greater inequality in a given country than an "autocratic" model would, nor that equality-minded regimes are more "autocratic" than "bourgeois" ones. To the extent that the categories are suggestive, some countries that come to mind have moved into the second phase *ab initio* while others have moved from a second-phase regime (e.g., populist) to a first-phase one (e.g., autocratic).

In any case, the models appear to be quite incidental to the authors' main con-

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cern, which is to demonstrate that political participation can be contained at a relatively lesser cost than was earlier anticipated. Migration studies, to which Joan Nelson has contributed, are reported to have exorcised the troublesome ghost of revolutionary minded urban and rural masses conjured up at the beginning of the 1960s. The rural masses, if left alone, choose "exit" over "voice" by moving to urban areas; having become urban poor of recent vintage, they are less revolutionary than older urban residents (p. 110). As it is easier to prevent the rapid extension of participation by refraining from providing the "context" which alone fosters it than to reduce participation once it has expanded, the technocratic alternative is quite feasible; and if it is feasible is it clearly preferable than the populist alternative which leads, in the view of coauthors, to nothing but decay? The choice is easy. The message from Harvard to Washington is clear: Let the sleeping poor lie.

ARISTIDE R. ZOLBERG
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On Revolt: Strategies of National Liberation *by J. Bowyer Bell. Cambridge, Mass., Harvard University Press, 1976.—xii, 272 pp. $15.00.*

Dr. Bell's work aims "to focus on the application of strategy, the orchestration of revolt in the name of national liberation, particularly of a special group of rebels who rose against the British Empire after World War II" (p. 17). He compares seven cases: the Irgun campaign in Palestine, the campaign for Gold Coast independence, the communist insurrection in Malaya, the Mau Mau in Kenya, the Fedayeen in the Canal Zone, EOKA in Cyprus, and the NLF in South Arabia. The Provisional IRA also receives considerable discussion throughout, and is accorded a section to itself. This book does its best to foster the illusion that the IRA is the latest in a long line of "Robin Hood" liberators fighting British oppression. And in case the reader misses the point it is trumpeted on the dust jacket, where Bell is pictured on the back posing with Sean MacStiofain, while on the front a British soldier crouches beneath an IRA slogan.

Bell starts encouragingly enough with an interesting analysis of the nature of revolt and a helpful typology of strategies of armed struggle (pp. 14–17). Thereafter, however, the book goes badly awry. No fresh material is provided in the case studies of revolts. The historical background, except in the case of Palestine, is hopelessly overcondensed and oversimplified. British policies are made out to have been uniformly blind and bad. No credit is given for the fact that major ethnic conflicts vitiated and obstructed British attempts to find acceptable political formulas for independence in many cases.

At times objectivity peeps through, as when Bell confesses that, on occasion, the banner of national liberation may be stolen by "driven or dreadful men seeking authority for violence" (p. 9). But nowhere does he admit that it is governments that have to deal with such disorders. The terroristic and vicious face of certain

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rebel movements is almost totally veiled or ignored. And this major sin of omission is enough to condemn the book as a serious piece of academic analysis.

But there is worse to come. It seems that Bell has become so captivated, if not mesmerized, by the myth of the terrorist, that he has begun to talk like a terrorist without critical awareness of the implications of what he is doing. For example, he asserts: "As every rebel recognizes, if he is to reach the bargaining table he must bomb his way there. All the subtle questions—when, where, and how the bombs go off, the size and importance of the target, the horror of the moment—matter not at all if the bombs do not go off. They are the rebel's key card and it must be played over and over" (pp. 229–230). If Bell really has come to believe that bombing the innocent is a legitimate means of political bargaining then it would have been better had he stayed at home reading history and philosophy rather than hob-nobbed with terrorists around the world.

PAUL WILKINSON
University College, Cardiff

The Politics of Deference: A Study of the Mid-Nineteenth Century English Political System *by David Cresap Moore. Hassocks, England, The Harvester Press, 1976.—529 pp. £15.50; New York, Barnes & Noble, 1977.—$19.50.*

As a concept, deference is more successfully postulated than defined. As a practice, it is more easily assumed than analyzed. Historians have earnestly grappled with the problem, but without satisfactory result. Sociologists and self-styled political scientists have contributed a multisyllabic vocabulary, but few insights.

The phenomenon was given expression and currency by the writings of Walter Bagehot, the "Darwin of Deference" as Geoffrey Best has felicitously dubbed him. According to Bagehot, England traditionally functioned as a "deferential community," whose participating members instinctively recognized and acceded to the superior leadership of their social betters. This "cake of custom," however thin, survived unbroken through the mid-Victorian years. D. C. Moore, subjecting Bagehot's theory to rigorous and sometimes intemperate appraisal ("This is patent nonsense" [p. 12]), has effectively crumbled that metaphorical cake into an assortment of hardtack biscuits.

The author displays a prodigious industry and a sharply critical intellect. But his ideas are ill served by a cumbersome structure and by a prose style of almost impenetrable turgidity. Moore has already established himself as a gifted essayist, whose contributions to scholarly journals have challenged and significantly modified professional opinion. Indeed, the most fluent sections of this book are those adapted from previously published articles that heralded the present major work. Long in coming, it sadly proves less than the sum of its parts.

Bagehot and "various others" are faulted for having "substituted an intellectual for a social dynamic when they tried to explain mid-century electoral behavior" (p. 13). This tendency is "more important for what it reveals of the logic of mid-century

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discourse than for what it reveals of mid-century realities" (p. 250). Without necessarily disputing the hypothesis, one may suggest that Moore's own perspective—and certainly his methodological gyrations—are no less the product of modern convention.

In the process of "demythification," Moore has made extensive use of poll books, which he acknowledges as a fragmentary and otherwise imperfect index to electoral recruitment. To the greatest extent possible, he relies on these data, supplemented by extracts from private correspondence, to illuminate the structure of interlocking "deference communities" and the subtle changes in orientation among them. "Deference was not the consequence of generalized relationships among the members of different classes," as Bagehot, for one, described. "Rather, it was the consequences [*sic*] of specific relationships among the members of specific groups and networks" (p. 446).

The argument is a sophisticated one that demands serious consideration by any student of nineteenth-century politics and society. Yet, as presented, it is tenuously linked to the more provocative assertion that the Reform Acts were intended to control or even counteract the forces of change by attempting "to increase the strength of the traditional social nexus by defining and redefining the various constituencies in terms of the traditional groups and communities" (p. 430). To support his second, more controversial claim, Moore has assiduously compared successive versions of each Reform Bill, laying stress on the permissive clauses and on the underlying "conditioning or triggering *relationships*" (p. 183). Needless to say, he does not regard either as "a Radical measure," but instead emphasizes the mutual concerns of reformers and antireformers. Far from aspiring to qualify as "proto-democrats," the architects of 1932 and 1967 are alike portrayed as anxious to avoid any distribution of power. It remains doubtful, however, whether Grey or Disraeli were as adept as Moore in interpreting the poll books.

In marshalling his evidence and evaluating its implications, the author makes virtually no concession to his reader. Example is piled on example, with the effect that the forest is obliterated by the trees. To make matters worse, the passage is encumbered by a good deal of verbal stumpage. Tautologies and paradoxes, some helpfully labeled as such, defy comprehension. Qualifications are often further qualified to a nullity. The safeguards "presumably" and "ostensibly" appear so automatically that they disfigure the text and vitiate the argument. And it is Moore's annoying habit to begin one sentence after another with the words "clearly," "obviously," or "undoubtedly," as if to excuse himself from the obligation to explain. Consequently, we are left to accept his case largely on trust. To defer or not to defer, that is the question.

STEPHEN KOSS
Columbia University

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The Model Ombudsman: Institutionalizing New Zealand's Democratic Experiment *by Larry B. Hill. Princeton, N. J., Princeton University Press, 1976.—xviii, 411 pp. $18.50.*

Fears have been expressed that "ombudsmania" is rampant. Ombudsmen have in recent years reared their benign heads in a hundred-odd places as diverse as Fiji, Great Britain, Iowa, and Dayton, Ohio. Proposals have been made for a federal United States ombudsman and for a United Nations High Commissioner for Human Rights, a sort of international ombudsman. Much of the credit for the current spread of this institution, first invented in Sweden in 1809, must go to Sir Guy Powles, the first English-speaking ombudsman who took office in New Zealand in 1962—the model ombudsman. The legislation constituting his office (based in turn on the statute instituting a Danish ombudsman in 1954) has been much plagiarized by subsequent law drafters elsewhere. Sir Guy's reports, speeches and articles have given wide publicity to the office. So have academic commentaries such as Walter Gellhorn's excellent chapter in *Ombudsmen and Others: Citizen's Protectors in Nine Countries* (Cambridge, Mass., 1966). Much of the academic work has, however, been primarily descriptive, based on relatively slight contact with the day-to-day working of the office and with the New Zealand administrative and political scene into which this institution has been transplanted. There has, in short, been a crying need for a detailed study of how the institution works, its strengths and weaknesses, and how well the patient and the new organ survived the transplant.

Larry B. Hill's outstanding book fills this need. He approached the subject from an anthropological perspective, much influenced by the techniques used by Malinowski in his work among the Trobriand Islanders. He devoted eighteen months in 1967 and 1968 to the new sport of "ombudswatching." He spent about thirty hours a week in the office observing the entire process, participated in staff meetings and discussed both general policies and the handling of specific cases. A large sample of cases was carefully coded and analyzed. All of the members of the New Zealand legislature, including ministers of the Crown, were interviewed, as were the permanent heads of nineteen major government departments. The author's synthesis of all this appeared originally as a Ph.D. thesis in 1970. The thesis material has been substantially rewritten and sharpened for the present book. It has been integrated with extensive additional data collected from Powles's annual reports up to March 31, 1975, and correspondence with civil servants and academics.

The book is, itself, a model. The research design was carefully honed to the subject matter, the scholarship is meticulous, and the writing is precise. The empirical data is imaginatively presented (especially the attempt to follow Malinowski's *dictum* that "the results ought to be tabulated into some sort of synoptic chart"). Space precludes discussion of Hill's findings in detail, but his most interesting discovery was that 72 percent of the complaints to the ombudsman are what he terms *"offensive."* Many early supporters of the ombudsman idea assumed that most of the work would be defending traditional civil and political rights against bureaucratic encroachment. While it is true that a significant part of the model ombudsman's work

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involved just this, most complainants are in fact trying to *extract* something from government—a social welfare benefit, a scholarship, compensation for injury. The ombudsman appears, at least in New Zealand, a country with a strong commitment to protecting its inhabitants from womb to tomb, much more an agency for the promotion of economic and social rights than for the protection of civil and political ones.

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Belgium and Europe: A Study in Small Power Diplomacy *by Jonathan E. Helmreich. The Hague, Mouton, 1976.—xiii, 451 pp. $24.00.*

As a small state operating in an international system dominated by great powers, Belgium has tried to preserve its independence and to remain detached from military conflicts among major states. This has not been easy. Situated between hostile powers, Belgian neutrality, and, therefore, its security has been chronically precarious. It did survive the Franco-Prussian War with its neutrality intact; but the inadequacy of formal neutrality as a defense against aggressive states was tragically demonstrated by the German invasions of 1914 and 1940. Belgium is an ideal subject for an examination of how a small, highly vulnerable state attempts to cope with its insecurity by protecting itself against aggressive enemies without being subordinate to powerful allies.

But does a small state such as Belgium, with limited military resources, have control over its own fate in the international system? Or, as Hans Morgenthau suggests, does the independence of small states hinge entirely on the aspirations of and power relations among the great powers? Morgenthau's view makes Belgian diplomacy irrelevant. Not surprisingly, Jonathan Helmreich, in his fine history of the evolution of Belgian diplomacy from 1830 to the present, does not endorse this view though his work is guided by Morgenthau's realist, power-politics approach. While Helmreich recognizes that Belgian diplomacy was shaped and limited by the great powers, he argues that Belgium made, and had the opportunity to make, important, independent diplomatic decisions which affected its security. For the most part, he successfully demonstrates the relevance of Belgian diplomacy.

This is the case, for example, in his discussion of the Belgian reaction to the Franco-Prussian War. In 1870 King Leopold II requested England to defend Belgium against any possible invasion by either of the belligerents in the Franco-Prussian War. Prompted, in part, by a draft of a secret treaty which indicated Napoleon III's intentions to incorporate Belgium into France, Britain asked the belligerents to sign a treaty pledging their intention to respect Belgian independence and stating that Britain would declare war on whichever state attacked Belgium. Belgium felt secure, and the Belgian leaders were delighted with this diplomatic success. But, as Helmreich stresses, this success was possible because of particular conditions of

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the international system; namely, Britain, along with Belgium, was a neutral in the Franco-Prussian War; Britain possessed sufficient power to influence the behavior of the belligerents; and Britain was willing to perform its role as balancer to prevent another power from gaining control over Belgium. But he also persuasively shows that Belgium did take the initiative to mobilize British support to protect its neutrality; Belgium probably bolstered its security through its diplomatic actions.

But Helmreich does not always demonstrate convincingly the relevance of Belgian diplomacy. This is true, for example, in his discussion of the relevance of Belgian diplomacy in the decade prior to World War I. Conditions were not nearly so propitious for Belgium during this period primarily because Britain had changed from a benevolent, neutral protector into one of the belligerents in the conflict between the Entente and the Triple Alliance. Though frightened by Germany, Belgium could no longer rely on Britain as impartial protector. Aid from England could compromise Belgium's independence by making it subordinate to the Entente powers. And Germany could justify an attack if Belgium became too closely associated with the Entente.

Helmreich sympathetically describes the dilemma Belgian leaders faced. Yet he suggests that Belgium might have altered its fate if its leaders had acted differently. Thus he complains that inadequate Belgian military preparation buttressed the German intention to implement the Von Schlieffen plan which called for the invasion of France via Belgium. But besides noting that "Von Moltke confidently wrote that the Belgian army would be easily scattered unless it withdrew to Antwerp" (p. 167), he offers no evidence that the Germans might have altered their basic strategy, which was intended to defeat France and Britain, if Belgium had improved her military capability. It is just not a very plausible position.

He also criticizes the legalism that characterized Belgian diplomacy during this period; "They [Belgians] expected international law and order to defend them but displayed confusion in considering how they in turn would defend international law" (p. 143). Yet in his detailed narrative of this period, he underscores all the difficulties involved in an abandonment of strict adherence to neutrality in favor of closer alignment with England. Furthermore, he does not spell out any feasible alternatives to the policy the Belgians actually pursued. It appears that, given the ambitions and behavior of the major powers, there may have been little the Belgians could have done to alter the course of events.

But this difficulty in Helmreich's analysis is minor in the light of his substantial contribution to the study of Belgian history and politics; he has written the most comprehensive and detailed history of Belgian diplomacy available in English. He clearly and carefully traces the development of Belgian foreign policy from the tumultuous decades following the 1830 revolution to the period of international cooperation following World War II. He also includes informative sections on Belgium's relationship with the Congo. In all, this is a valuable book not only for

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students of Belgian politics and history but also for scholars concerned with how small states behave in the international political system.

JEFFREY OBLER
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Germany and the League of Nations *by Christoph M. Kimmich. Chicago, University of Chicago Press, 1976.—x, 266 pp. $17.50.*

Succinctly written and based largely on a wealth of published and unpublished documentation, Kimmich's study will not likely be superseded in the foreseeable future. The title itself is somewhat deceptive for his work is meant to be more than an investigation of Germany's short-lived involvement with the League of Nations and the system of international cooperation that it was meant to generate in Geneva.

The book is chronologically and topically structured. The first chapter concerns itself with Germany's initial attitude toward the idea of a League of Nations in 1918–1919. Subsequent chapters trace the evolving German attitude toward the Geneva organization based on internal German political considerations and the exigencies of international politics, terminating with Germany's admission to the League in the autumn of 1926. The latter half of the book tackles the topical issues that touched Germany's vital interests and monopolized its attention once it was admitted into the world organization in 1926, namely, reparations, the minorities problem, the disarmament question, and finally Nazi Germany's decision in the autumn of 1933 to withdraw from the League.

If there is a thread running through this clearly written and analytical volume it is that Germany's policy toward the League was not "internationalist," regardless of whether this policy was orchestrated by Gustav Stresemann or by his predecessors and successors, and regardless of whether or not they were committed to the democratic system of government of Weimar Germany. It is not open to question, as Kimmich contends, that Berlin's League policy was meant to further German interests by using the Geneva organization. But then again what state during the interwar period of the League, or for that matter in the United Nations today, was so altruistic as not to use the Geneva organization to further what it considered to be vital national interests? Certainly Paris' attempted manipulation of the world organization was no more "internationalist" than that of Berlin. The saving grace for the French was that they were on the winning side in 1918. France's policy stance also applied to all those states committed to the status quo, especially in Eastern Europe. In London, any internationalist orientation was restricted to the League's mediatory and conciliatory procedures assuming that no vital British interests were threatened. If one wishes to argue a "continuity theory" in German foreign policy, as Kimmich does, perhaps it might be wiser to differentiate between the qualitative

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and quantitative desires of German governments partially reflected in their policy attitudes toward the League.

It was obvious following France's ill-fated occupation of the Ruhr in 1923, that at some point Germany would again have to be integrated into the European political community. Any such move would necessitate a far-reaching readjustment of the Versailles settlement. German pressure to bring this readjustment about as quickly as possible, it can be argued, either through bilateral negotiations or through the League system, was a push for a negotiated revision of the status quo established at Paris in 1919. This move can be viewed as realistic and not unreasonable in view of Germany's former role and potential power in world politics, rather than as an attempt to undermine or overthrow the legal and political order established by Versailles. The former procedure, to revise the Versailles settlement following the Ruhr occupation, was almost an inevitable and realistic readjustment accomplished in part through the League's machinery. The latter procedure, to undermine and overthrow the Versailles settlement, which soon commenced with Germany's withdrawal from the League, can be perceived as part of an unswerving German foreign policy line and Kimmich has chosen to view it so, as he cogently argues in his conclusions. Kimmich's interpretation, however, is not one to be lightly dismissed, reflecting as it does an expertise on German foreign policy initially established by his fine study of Berlin's policy toward the free city of Danzig in the period 1919–1934.

JAMES BARROS
University of Toronto

In the Name of the People: Prophets and Conspirators in Prerevolutionary Russia *by Adam B. Ulam. New York, The Viking Press, 1977.—xii, 418 pp. $15.00.*

Few events of nineteenth-century European history can match the rise of Russian revolutionary terrorism in drama, psychological mystery, and political implications. It has attracted the attention of novelists (Dostoevski), essayists (Camus), moralists (Berdiaev), and historians (Venturi). The fascinating story which culminated in the assassination of Emperor Alexander II has been told more than once in English, as well as in other languages. But the story is so compelling that it bears retelling, especially when it is done with such literary skill, psychological insight, and scholarly care (though some purists will complain that an occasional sentence goes beyond the letter of known documents and scholars will deplore the omission of the original Russian titles in the footnotes) as in the present book. But Mr. Ulam also makes valuable and original contributions to both the historiography and our understanding of the prologue to the Russian revolutions of 1917 and their aftermath.

The historiographic tradition of Russian revolutionary terrorism has generally displayed two main features: First, it is sympathetic to the terrorist groups and movements as positive contributions to the struggle for liberation from autocracy, extolling the participants' heroic abnegation and high moral character (in spite

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of occasional individual lapses). Second, it has taken seriously the alleged ideological contents of the propaganda of Russian revolutionary Populism and terrorism after 1861. Historians and publicists have created an "apostolic succession" of ideas and movements which led up to 1917, and have retrospectively endowed the history of the Movement of Liberation with a kind of inevitability. This has been a characteristic feature of liberal and Marxist, as well as Soviet, writings on the subject, whatever their disagreements on particulars.

Ulam breaks with this mythologizing in order to clear the way for a proper understanding of the behavior and motives of the actors and to draw the political lessons inherent in the story of revolutionary terrorism in Russia. He takes the ideological pronouncements of the terrorists with less than reverential seriousness —and rightly so. He shows up both their inconsistencies and political naiveté which had such far-reaching and unexpected consequences. At the same time Ulam stresses the exemplary role of the personalities, for indeed it was the psychopolitical aspects of the movements' participants that led to revolutionary terrorism and set the pattern of political behavior which dominated Russia from then on, indirectly giving its particular shape to bolshevism. Ulam brings out these features with great skill: he cuts down the alleged heroes and villains to their true humanity (e.g., pointing to the unbalanced and criminal personality of Nechaev or vindicating the intellectual and organizational talents of Lev Tikhomirov) and he stresses the consequences the revolutionaries' ethical nihilism and political amorality had for their tactics, which set an example for Lenin and his Bolsheviks (e.g., see p. 329ff).

No historian of the Russian revolutionary movement can avoid retrospectivity. In this respect, too, Ulam has done a splendid job. He never misses the opportunity to point to the consequences, parallels, or contrasts the story of revolutionary terrorism offers for an understanding of events in the Soviet Union or most recently in the West, and he does it with much wit, irony, and perceptiveness. The relevance and contrast revolutionary terrorism holds for an understanding of 1917 and the Soviet system are less challenging than are Ulam's remarks on the parallelism between the atmosphere and events of nineteenth-century Russia and the ferment and disarray in the West since the 1960s. Departing from historiographic orthodoxy, Ulam splendidly documents and dissects the reactions of Russian society to the rise of revolutionary terror. He points out that the moral obtuseness of Russian public opinion in fact encouraged crime for fear of criticizing young radicals, and he convincingly argues that, more than the acts of terrorism themselves, this stance created the political impasse which led to the conflagrations of 1905 and 1917. Not that Ulam is blind to the shortcomings of the government; unlike most historians less attuned to the complexities of politics, he points out that the government's foolish reactions (too lenient or too harsh at the wrong moment) were as much responsible for the successes achieved by the terrorists as the country's objective circumstances and the ideological claims of the revolutionaries.

Perhaps Ulam can be faulted for exaggerating the relevance of the revolutionary

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terrorism of the 1860s and 1870s for bolshevism later. But he is surely right in arguing that the "self-fulfilling counterprophecies" which informed the incoherent and paradoxical responses to organized terror on the part of both public opinion and the government paved the way for the ultimate triumph of an unscrupulous elitist party. The cautionary aspects of this story for our own time are clear. Ulam's splendidly readable account of the Russian experience may ensure that they will not be lost.

MARC RAEFF
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International Crisis: The Outbreak of World War I *by Eugenia V. Nomikos and Robert C. North. Montreal, McGill-Queens University Press, 1976.—xvi, 339 pp. $23.00.*

In 1957 Robert North of Stanford embarked upon a series of conceptual and descriptive studies on the start of World War I. Since then he and his associates have produced numerous articles and three major monographs. This volume, done jointly with Eugenia Nomikos, represents the completion of these labors. A brief review cannot attempt a detailed assessment of this enormous effort. Yet it is possible that many of the strengths and weaknesses of this final study will also come to form a part of a larger assessment of this work.

Unlike the other two monographs, this one is principally historical. Of fifty-nine chapters, only six are conceptual. As an historical account of July 1914 the work is at once clear and concise, old-fashioned and helpful. Defining crisis as a threat to a state's vital interests in which time and surprise necessitate a response, the authors examine—capital by capital—the European reaction to the murder of Franz Ferdinand at Sarajevo. In this the focus is almost exclusively narrative, with only occasional analytical excursions; indeed not until the end of the volume does any substantial historical interpretation emerge. The work is based on the published diplomatic documents and the standard, pre-1960 historical accounts. The work of Fritz Fischer and his students, though cited, has not significantly influenced the view of events, nor have the more recent studies on Britain, France, or Austria-Hungary. In a sense, therefore, the confines of the initial project—with its heavy emphasis on the published diplomatic documents—remain apparent here.

Nor do North and Nomikos really bridge the gap between the 1914 crisis and the longer-range dynamics examined in the Nazli Choucri-North study, *Nations in Conflict*. What the authors recognize (as did Ole Holsti in his companion, *Crisis Escalation War*) but never overcome is the linkage between structural developments in the international system and the immediacy of crisis decision making. Nowhere, for example, do the tensions of central Europe and the Balkans in the years 1912–1914 receive adequate attention; this in turn almost necessarily makes the description of perceptions in St. Petersburg, Vienna, and even Berlin less than satisfactory. Yet the historical account remains comparative and balanced. Vienna exists as a

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separate entity; Russia is made responsible for escalatory action; and the illusions of London and Paris, as well as Berlin, are clearly displayed. If the Central Powers were the major initiators in the crisis, they did not soon lack for companions in a process where "scarcely a single major move calculated to achieve a particular outcome was to any appreciable degree successful" (p. 30). And everywhere the war-peace paradox—of steps taken to reduce tensions having in fact the reverse impact—was relentless. On the whole, therefore, while the specialist may find fault, this is a valuable and useful summation of the 1914 crisis.

By contrast, the conceptual contribution is less impressive. Indeed it has a curious add-on quality. Categories, terms, approaches, all are mentioned but not systematically linked to the historical chapters. And the one attempt to do this (Chapter 57) lacks clarity and incisiveness. Even less clear is how the Atkinson proposition (p. 251) on individual activity helps rather than obscures the authors' theoretical discussion. At one point they seem to embrace the "bureaucratic politics paradigm" only to back away (p. 254). What is missing is a linkage between the discrete and historical with normative propositions that would add value to the theoretical section. Still, their concluding maxims for statesmen are worth noting and affirming.

But their last injunction—that statesmen transfer conflict situations into mutually acceptable compromises—requires special comment. While a desirable goal, this admonition seems singularly inappropriate as the conclusion for a study on the origins of World War I. For few crises have been so strikingly devoid of that possibility. What the decision makers in Vienna saw at stake that summer of 1914 was the very existence of their multinational state. If Serbia remained unpunished, the Danubian monarchy would no longer be a great power and would soon face Turkey's fate. In this tragic situation all concessions seemed intolerable and inadequate. If such was the case in the relatively nonideological world of pre-1914, how much more dangerous are the chances for noncompromise situations in the late 1970s. Robert North and his colleagues have pointed out the dangers of 1914 and the processes of escalation; it can, however, only make us uncomfortable about the future.

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Mao Tsetung and China *by C. P. Fitzgerald. New York, Holmes & Meier Publishers, 1976.—vi, 160 pp. $9.50.*

The academic marketplace hardly abounds in biographies of the late Chinese Communist party chairman Mao Tse-tung, but most of the studies that are available have been written not only by China experts but for them. Edgar Snow's interviews, conducted in 1936 and published in *Red Star over China*, are the exception to this general rule and remain the classic account of Mao's early life. But in the forty years that intervened between Snow's interviews and the death of the party chairman, Mao Tse-tung not only led his party to victory over the mainland but engi-

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neered a radical transformation of Chinese society. Classic though they remain, Snow's interviews cannot provide an interpretation of what was to follow. Stuart Schram's more recent *Mao Tse-tung*, is an intellectual *tour de force* of truly impressive proportions. But the sheer detail of Chinese names, places, and historic events is bound to leave the uninitiated at sea. For both the general reader and the introductory student, then, C. P. Fitzgerald's *Mao Tsetung and China* serves as a welcome corrective. Completed before Mao's death, the book provides a thorough and easily understandable introduction both to Mao Tse-tung and the revolution he led.

The primary virtue of Fitzgerald's book is its prose. Because it reads more like magazine journalism than standard academese, the reader is quickly absorbed into the unfolding drama of China's revolutionary process. Nor is the reader overburdened with Chinese names and places; they are introduced only when necessary and then with ample explanation and background.

Readers who remember Fitzgerald's earlier study, *The Birth of Communist China*, will be interested in the contrast. *The Birth of Communist China* was founded on the premise that China is and must somehow remain changeless, that "the Chinese Revolution has ended in a new version of the ancient society" (p. 42). In *Mao Tse-tung and China*, however, Fitzgerald not only concedes China's revolutionary transformation but has clearly come to admire it. While Fitzgerald is by no means alone in his conversion, he sometimes unnecessarily allows his admiration to overwhelm "best guesses" about postliberation politics. For instance, in noting the executions that took place during land reform, Fitzgerald cites the tentative estimate of a British diplomatic official of 100,000 to 150,000 killed (p. 76). While accepting Fitzgerald's normative perceptions of land reform, most American scholars would agree that the figure is probably closer to 1 to 3 million. If Fitzgerald feared that such a figure might seem overly high, he need only have pointed out that even the 3 million figure represented only slightly more than one-half of 1 percent of China's population, that it had not been unusual for that many and more to die of starvation in a single year, and that some 20 million Chinese had died during the course of the Taiping Rebellion in the mid-nineteenth century.

China experts will bridle at some of the factual inaccuracies in Fitzgerald's book. The Soviet Union was not "inexplicably absent" (p. 87) from the Security Council meeting that brought the United Nations into the Korean War. They had absented themselves to protest the world body's refusal to seat the People's Republic of China. Wu Han's "Hai Jui Dismissed from Office" was criticized in 1966 but had been written several years earlier (p. 115). And Mao Tse-tung had not "always enjoyed swimming" (p. 115); by his own admission, he learned to swim quite late in life.

But these flaws should not overly detract from the primary merits of the book. Fitzgerald writes with lucidity, sympathy, and simplicity, thus rendering an understanding of China accessible to a far wider audience than the more ponderous studies designed to appeal exclusively to the experts.

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The Man Who Lost China: The First Full Biography of Chiang Kai-shek *by Brian Crozier, with the collaboration of Eric Chou. New York, Charles Scribner's Sons, 1976.—xv, 430 pp. $12.95.*

Given the truism that revolutions cannot be understood apart from the forces they oppose, it is high time there appeared a critical biography of Mao Tse-tung's arch opponent, Chiang Kai-shek. This book, however, only partially fills the bill. While it may be, as the title claims, the first full-length biography of the Nationalist leader through his death in April 1975, it cannot adequately explain the reasons for the other half of the title, how the components of Chiang's personality and leadership facilitated his "loss of China." Chiang did lose power, but a country as huge and diverse as China clearly was far beyond the grasp of any one man. A problem with the book therefore is inherent in its attempt to capture the history of an epoch through the life of a man. Neither the man nor the epoch is fully realized.

The problem, but also the challenge, especially for writing about vast and inscrutable China, is to say something both comprehensible and meaningful. Perhaps a work concentrating more on Chiang Kai-shek the man, with greater attention to his personal qualities, as studied for example by Pichon P. Y. Loh (*The Early Chiang Kai-shek: A Study of His Personality and Politics, 1887–1924* [New York, 1971]), might come to terms better with what made the man tick. But such a study by definition assumes wide knowledge of the history of the country. Therein lies the rub and a fundamental problem for biographers of leaders of strange cultures. To give discussion of wider currents of unfamiliar history adequate for the general reader leaves little room for in-depth study of the presumed subject of the book. Thus in Brian Crozier's biography, the reader is left somewhat breathless by continued jumping about among the elements of twentieth-century Chinese history, from Manchus to warlords, from Nationalists to Communists, with the actions of Japan, Russia, and the United States thrown in, and with inadequate attention left for the ostensible subject of the book.

Perhaps these problems are unavoidable given the complexities involved, and having been mentioned, should not obscure some real values of the biography. It is indeed the first overall account of Chiang Kai-shek's life, immersed as it is in a somewhat sketchy narration of broader problems. In general, the account gives sound interpretations of crucial developments. One argument, as interesting as it is controversial, holds that "the greatest of Chiang Kai-shek's many errors of judgment" was his refusal to fight Japan after the Mukden incident of September 18, 1931 (p. 145). The author holds that had Chiang done so, his people would have rallied around him and he might thereby have survived as the leader of greater China. This is a plausible interpretation, but the author having brought up such a "might have been" should have discussed at least briefly some counter arguments, such as the fact that China probably would have been overrun entirely by Japan ten years instead of four before Pearl Harbor, a presumably crucial point. Crozier perhaps also overstates the degree to which United States actions, including the Yalta agreements with Stalin, and the controversial suspension of new arms aid in 1947, decisively affected the fortunes of the Nationalists. This tends to minimize the

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weakness inherent in Nationalist overdependence on foreign aid, the reverse of the earlier cold war formula that the Communists were the puppets of a foreign power.

Here and there throughout the book is some new information. There is a fuller account of some reasons Chiang's son, Ching-kuo, the present ruler of Taiwan, went to Russia in 1925 and stayed there until 1937, even as another son, Wei-kuo, went for schooling to Nazi Germany. These were simply a desire for foreign education combined with diplomatic reasons even though these experiences seemed to contradict Chiang's stated dislikes for Russia and Germany, cited elsewhere in the book (e.g., pp. 62 and 177). There are other little-known details, for example, about the narrow escape from execution of Chou En-lai in April 1927 (through the intercession of a former student who was the brother of a Nationalist divisional commander). There is also information on the floods of 1931 and on the Nationalists' attempted drowning of part of the Japanese army in 1938, and there is the statement that Stalin invited Chiang Ching-kuo to Moscow in early 1946 with implications of new wrinkles in the Moscow-Nanking-Yenan triangle. There is also a good and relatively detailed account of the activities of key Nationalist personnel in the chaotic and decisive years of the final civil war, 1946–1949, as well as an adequate summary of the years in Taiwan. An important informant for many of these details and numerous anecdotes is Eic Chou, author in 1963 of *A Man Must Choose*, his own account of experiences in the 1940s and in Communist China after 1949. The collaboration between the two writers serves this book well.

The principal author's final conclusion is that Chiang Kai-shek was "not only a deeply flawed leader, but in the classical sense of the Greek tragedies, a tragic one" (p. 399). Even though Chiang "never for a moment doubted his claim to be the rightful heir to Sun Yat-sen's Revolution" (p. 130), in the final analysis, he was both unlucky and flawed. He was especially unlucky to have his attempted rule coincide with the peak of Japanese expansion, and to face an opponent of the stature of Mao Tse-tung. Of course Chiang would never have come to power in the first place but for the death of Sun Yat-sen and the nature of the challenge of defeating the warlords. But Chiang "had virtually no understanding of the social bases of permanent power" after 1927 (p. 398). For such insights and a useful overview, the author is to be commended. What is needed now is a more profound analysis of the relationship of Chiang's personality to the problems China faced from 1927 to 1949.

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How Japan's Economy Grew So Fast: The Sources of Postwar Expansion *by Edward F. Denison and William K. Chung. Washington, D. C., The Brookings Institution, 1976.—xvi, 267 pp. Cloth, $10.95; paper, $4.95.*

Two major questions have been a source of fascination to analysts of Japan's postwar economy: How has Japan managed to achieve such remarkably high growth

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rates and how long can she keep it up? Drs. Denison and Chung have provided a revealing and meticulously researched statistical approach to the answers to these questions. Their book is one of the products of an unusually ambitious research project on Japan's postwar economy conducted by the Brookings Institution. The results have already been reported in thirteen separate essays contained in a formidable 1976 volume, *Asia's New Giant*, edited by Hugh Patrick and Henry Rosovsky. One of these essays is by Denison and Chung. The present book is a substantially expanded version of their essay and includes a lengthy appendix explaining in great detail the statistical methods and sources used in arriving at the numerous quantitative measures employed in the analysis.

The methodology was developed by Denison in three earlier studies which analyzed the sources of the postwar growth rates of various Western countries. Chung applied Denison's "growth accounting" techniques in a previous study of Japan's postwar economic growth. The methodology consists of first breaking down the sources of growth into two basic categories: total factor input and output per unit of input. The contribution in percentage points to the national income growth rate is determined for each of these categories in proportion to their individual growth rates. Each of the two categories is broken down into subcategories. Factor input is separated into the classical divisions of labor, capital, and land. Output per unit of input is subdivided into four contributing factors: advances in knowledge, improved resource allocation, economies of scale, and irregular factors such as the weather. Where applicable, subcategories are subdivided further. In all cases, the percentage point contribution to the aggregate growth rate is estimated. The analysis is performed for the time period 1953 to 1971, with a further breakdown into the periods 1953–1961 and 1961–1971.

The first five chapters are primarily summaries of methodology and statistical findings, but they contain some unusually valuable comparisons with the United States and other Western countries. The next five chapters provide details of the estimation procedures and results for the various factors contributing to growth. Many of the findings, such as Japan's structural similarities to Italy, confirm what has been observed by others. There were some surprises, however. Wage-setting conventions regarded as unique to Japan are sometimes assumed to result in unusual wage patterns. But Denison and Chung found that the patterns were very similar to those of Western countries. They concluded that the similarity "suggests that insofar as Japanese earnings are set by conventions the conventions tend to codify differentials that would be present in any case" (p. 30). Much has been made of the impact on productivity in Japan of the rapid shift out of agriculture into high productivity sectors, but the authors' estimates indicate that this accounted for only about 13 percent of the growth in overall output per unit of input for the whole period under consideration. Several Western countries experienced higher relative contributions from this source. The whole question of productivity differentials is given an interesting treatment in a chapter showing in quantitative terms for 1970 the extent to which various determinants are responsible for a lower level of labor productivity in Japan than in the United States. Comparisons are also made with

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other Western countries. The final chapter answers the question, "Can the growth rate be sustained?" in the negative. The rate of over 9 percent for 1961–1971 is expected to trend downward to about 3 percent by the year 2000.

From the book's analysis a good quantitative picture emerges of how Japan's growth was possible. A question still to be answered convincingly is why the possibility became reality. It is here that we leave the relatively secure world of statistics and enter the more nebulous realm of cultural institutions and motivations. Economics provides part of the explanation for Japan's rapid growth. Other disciplines will have to provide the rest.

ALAN H. GLEASON
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Legislative and Political Development: Lebanon, 1842–1972 *by Abdo I. Baaklini. Durham, N. C., Duke University Press, 1976.—xv, 316 pp. $11.50.*

This is a book about the role of the Lebanese Parliament and its contribution to the political system of the country. It is an easy book to read and follow. With its numerous illustrative tables, many sections are extremely informative about the nature of relevant socioeconomic, religious, and political concepts of the confessional system of Lebanon.

In the first part of the book, the author provides a narrative framework of analysis for the study of political development and the role of legislative bodies. The approach, according to Baaklini, "is not meant to generate the grand theory of social and political change, the dream of social scientists. . . . Furthermore, [the] study is not intended to generate testable propositions, although many such propositions can be found in the text for those interested. The approach . . . is basically intended for understanding and explicating the political development . . . and the role that the political actors assigned to the legislature in this development process. This approach is comparative in nature . . ." (pp. 29–30).

The work of Baaklini is, for the most part, a history of events in Lebanon since the Imarah period (1590–1842) to the 1970s, with emphasis on 1842–1972. The author also gives a balanced and excellent analysis of these historical events, of Lebanese national feeling, of the confessional system, and of Lebanese society. The book gives valuable information about Lebanese leaders, families, clans, religions, and "boss" politics which brought about the complexities of the political system and the role of the legislative body.

Baaklini analyzes the role of the legislative body in Lebanon under six headings: (1) symbolic function, (2) integrative function, (3) regulative function, (4) champion of national liberation struggle, (5) incubation of opposition, and (6) government-voter linkages.

Historically, the feudal system and the Maronite Church were the two main institutions that led to the emergence of legislative bodies in Lebanon. The Maronite Church was mainly responsible for the peasant revolts of 1820, 1821, and 1858

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which led to the downfall of the feudal system and the emergence of a mercantilist class, the inception of modern bureaucracy, and the genesis of the modern legislative body.

Functionally, the legislative body under the French mandate system was responsible for providing the country with a constitution, resisting French rule, and fostering a political entity. Furthermore, the Parliament performed the essential task in regulating political succession which might otherwise have shattered the unity of the country. "As a guardian of the Constitution the Chamber has acted as a check to the temptation of the executive to manipulate the Constitution for its own political ends" (p. 270).

Regardless of the religious affiliations, the political actors in the legislative body were mainly responsible for structuring the principal tenets of the ideology of the Lebanese political and economic systems. The National Pact is an informal agreement to create the foundations that are essential for a viable nation. The legislative body was concerned with possibilities rather than abstract notions. In the words of the author: "While Lebanon has failed to create a consensus around 'nationalism,' it has succeeded in creating a consensus around a polity and a system of government" (p. 139). Such development was marked by the following characteristics: (1) a low profile of political party participation and (2) an increase in political participation and competitiveness by the people as manifested through the electoral process. Through its structure, the Parliament has acted as a linkage to discharge its functions, thus bringing the various groups and institutions of the country into the mainstream of political life.

One disappointment is that the book—though published in 1976—does not consider the various essential factors that could lead to societal disruptions, as in the current breakdown of the confessional system. This, however, is not the fault of the author. This comment is an observation rather than a criticism and should not detract from the central innovative qualities of the book. It is, as such, a positive contribution to the literature of comparative political development, a provocative and stimulating work, and highly recommended.

ENVER M. KOURY
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International Aid and National Decisions: Development Programs in Malawi, Tanzania, and Zambia *by Leon Gordenker. Princeton, N. J., Princeton University Press, 1976.—xx, 190 pp. $13.00.*

Drawing on his own international experience, Gordenker has set out to examine the interaction of international development organizations (in fact, the United Nations cluster) and the governments of countries receiving aid. His particular concern was to explore the interpenetration of international and national administrative structures and mutual influence between the two, focusing on three African cases.

Gordenker's findings on structural interpenetration are striking. Not only do

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international organizations provide expertise on specific projects, but they also provide personnel who assume formal, at times very senior, governmental positions. That facilitates some mutuality of influence, though that influence usually is limited to specific areas of common concern, rarely persists beyond the completion of the project, and depends heavily on the individuals appointed. That the long-term mutual influence on patterns of decision making is very limited, Gordenker argues, gives little support to the notion of spillover effects of functional cooperation.

Though Gordenker's report enhances our descriptive understanding of the interrelationships of international aid and national decisions, the flaws in his book limit his contribution to our analytic understanding.

First, Gordenker presumes the beneficence of the motivation of international organizations, a presumption certainly not shared by the recipient governments. By limiting his purview to the United Nations and related organizations he may be on more solid ground in his presumption of beneficent motivation, but in doing so, he loses sight of the impact of other nonnational organizations involved in the relationships he describes (ranging from the World Bank to Oxfam).

Second, this book is characterized by a curious mix of insight and apparent naïveté, leading to occasional contradictions. For example, Gordenker is very clear on the political nature of many decisions described as technical. Yet, at the same time, his institutional orientation leads him to underestimate the extent to which projects for which aid is sought originate in fact in the aid-supplying organization.

Third, Gordenker abstracts the United Nations from the larger structure of world politics, losing sight of the role of the U. N. as a policy arena and a policy tool for competing world powers. That the World Bank, in which United States dominance is clear, emerged as a major determinant of development programs in Malawi is neither accidental nor inconsequential.

Fourth, the work is weakened by its casual attitude toward evidence. Gordenker's reading and study on his three cases are very narrow and selective, with the result that his commentary is often superficial; little recent data supplement his field work of the 1960s. His anecdotal style of case study reporting is neither concise nor precise, and there is little attention to relevant supporting research.

Fifth, Gordenker works in a theoretically eclectic style. That both suits his preference to focus on the empirical experience and promises a fruitful intermingling of divergent theoretical orientations. Unfortunately, Gordenker's eclecticism turns out to be a strategy for avoiding difficult issues. Throughout the book, he snipes at theoretical insights, rarely engaging them substantially.

Most important, Gordenker does not attempt to come to grips with the significance of his findings. What difference does it make that Third World states can occasionally influence international organizations in specific project areas? What is the significance of the ability of international organizations to influence the course of change in the Third World? That is, who benefits from the relationship which permits aid organizations to make specific projects more or less likely to occur, according to criteria which differ from those of the recipient government? And to whose advantage is the provision of what turn out to be senior government advisors,

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apparently largely chosen from the West, with both narrowly technical and broadly political responsibilities?

Descriptive and reportorial, this book adds to our data base on the behavior of international organizations, and, only to a very modest extent, on problems of development planning in Africa. Would that it made a comparable contribution to our analysis of these phenomena.

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Power, Politics and Progress: Social Change in Rural Peru *by William Foote Whyte and Giorgio Alberti. New York, Elsevier Scientific Publishing Co., 1976.—xii, 307 pp. $15.00.*

Power, Politics and Progress is the result of a massive collaborative research effort between Cornell University and the Peruvian *Instituto de Estudios Peruanos* (IEP). At least twenty principal investigators made in-depth studies of some eighteen rural communities in four diverse regions of Peru. In contrast to the one-shot survey application of many studies of rural change, the Cornell-IEP team continued research for ten years, applying survey analysis in 1964 and 1969 and performing participant observation at various times. Further, authors Whyte and Alberti go beyond the conventional nearsighted focus at the village level, and relate changes in the communities to changes in the nation.

This innovative research effort bore considerable fruit. In contrast to many studies that provide a vivid but static picture of everyday life in rural Peru, this volume emphasizes the origins and trajectory of change in different kinds of peasant communities. The authors examine in rich detail how conflict over land ownership or political independence emerged in specific communities and how, in many cases, the majoritarian peasant group won the conflict. Whyte and Alberti conclude that conflict and cooperation should be perceived not as a single continuum but as a four-box typology; although conflict indicates factional strife in the community, it also means that groups are actively pursuing progress, whereas a site low in both conflict and cooperation is a "village going nowhere" (p. 235). Other conclusions are also drawn. The potential for rural revolution in Cuba is contrasted to the difficulties of its attainment in Peru, stressing for example the effectiveness of the Peruvian military forces and the rugged Andean terrain. Further, Whyte and Alberti advance practical recommendations to promoters of change, suggesting that promoters recognize that change projects entail distinct levels of peasant effort and distinct patterns of reward, and thus require different political strategies by the change agent.

As the range of the authors' conclusions suggests, however, the study lacks an integrating conceptual focus. Whyte and Alberti emphasize that they "abandoned the modernization framework and came to focus on *structural* change" (p. 5). Yet,

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the meaning of the "modernization" or "culture-contact" approach versus the "structural" framework is not clearly developed. The authors stress the stimulus to change provided by schoolteachers and urban migrants (pp. 122, 170, 184, 193, 244). Such an emphasis seems more appropriate to a "culture-contact" analysis than a "structural" approach. Even more important, a "structural" framework calls for discussion of government policy, but the role of the state is downplayed throughout the volume.

The minimization of the impact of government weakens the analysis of social change in rural Peru. Whyte and Alberti perceive peasant progress as a gradual, virtually inexorable process reflecting the decline in the land-owning elite and the peasants' achievement of new economic and organizational resources (pp. 200–201). This interpretation seems largely valid with respect to independent peasant communities and disadvantaged haciendas in Peru. However, the analysis does not comprehend prosperous haciendas, such as those on the Peruvian coast, where the landowners' power was great and state action was necessary to turn the haciendas over to the peasants. Sociopolitical change on these haciendas was limited until the 1968 takeover by the military and its agrarian reform program. Whyte and Alberti should have clearly indicated the omission of these prosperous haciendas from their study.

Despite these flaws, *Power, Politics and Progress* is a significant contribution to the study of rural social change. The book suggests that what social scientists so often call for—longitudinal research integrating survey, participant observation, and historical techniques, and based on a collaborative North American-host country team—can actually be done, and done effectively.

CYNTHIA MCCLINTOCK
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Why Marxism? The Continuing Success of a Failed Theory *by Robert G. Wesson. New York, Basic Books, 1976.—vi, 281 pp. $12.95.*

There was a time when Americans looking for information on Marx and Marxism were more likely to be referred to the works of the late J. Edgar Hoover than those of, say, Paul M. Sweezy. Over the past fifteen years, however, original works and new translations in this area have somewhat redressed the balance of bias and level of treatment. Restricting ourselves to Marx's Marxism, we might mention, together with dates of major publication, the contributions of Shlomo Avineri (1968), Lucio Colletti (trans. 1973), Raya Dunayevskaya (1958, 1973), Iring Fetscher (trans. 1971), Helmut Fleischer (trans. 1973), Dick Howard (1972), Nicholas Lobkowicz (1967), David McLellan (1973), István Mészáros (1970), Bertell Ollman (1971), and Alfred Schmidt (trans. 1971). If many of these scholars are favorable to Marx, we can allow Lobkowicz to speak for the dissidents among them: "Throughout this study I have refrained from criticizing. This should not be taken to mean that I consider most of Marx's ideas correct, for example. On the contrary, I consider them theoretically unsound and entail-

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ing truly tragic consequences if applied in practice. But it seemed to me that a faithful and objective exposition is usually the best criticism." (*Theory and Practice: History of a Concept from Aristotle to Marx* [Notre Dame and London, 1967], p. xiii.)

If Professor Wesson cannot be included in this distinguished list—not least because he has forgotten or has never encountered Lobkowicz's remarks—then, to be fair, it must be reported that few of those included in it are cited in Wesson's notes or index. Nor is their omission irrelevant in this context. Despite Wesson's disclaimer that he is not offering an "analysis of the doctrines, an endless and baffling task to which little attention is given here" (p. vi), he continually attacks this same "doctrine" for its lack of cognitive content. Thus, when he revives the old criticism of Marx's use of language —"slithery use of words equals sloppy thinking, self-deception, or politically useful argumentation" (p. 219)—he appears totally unaware of Bertell Ollman's critical discussion of that issue, as well as the same author's refutation of such claims as "Marxism suggests that man is basically a rather simple creature moved mostly and fundamentally by economic urges . . . " (p. 221). Nor are such assertions as "Marx wished to bring the process of realization of the historical Absolute into a material, hence supposedly sounder frame" (p. 14), troubled by the shadow of the extended debate on the Hegel-Marx relationship that has fallen across the pages of many of the authors mentioned. But perhaps Wesson's treatment of this and related issues is less of an incitement to consult such "secondary" sources than to read the original texts so treated. Finally, readers acquainted with Marx studies as well as Marx's writings will be interested to learn that he was a backward-looking romantic opponent of capitalism, as evidenced by such statements as that from the *Communist Manifesto:* "The bourgeoisie, wherever it has got the upper hand, has put an end to all feudal, patriarchal, idyllic relations" (quoted on p. 11). Similar characterizations of Marx's views are too numerous to list here.

Marxism after Marx is shown little greater respect: "the entire Marxist movement has been singularly lacking in original or creative thought since its beginnings" (p. 198), an assertion made easier to sustain by a few brief references to Georg Lukács and Antonio Gramsci and by ignoring the work of Karl Korsch, Max Horkheimer, and Theodor W. Adorno, as well as hundreds of other major Marxist intellectuals, scholars, and artists, from the East and West.

Why Marxism? Because Marxism is such an "obvious" intellectual failure, and because "the concerns of the world have changed" (p. 7), it must be traced in its persistence to other motives: "irrational political and psychological needs" (p. vi), "anger and antagonism" (p. 197), "a secular religion for those who have lost supernatural faiths" (p. 182), and subjective feelings of inferiority: "Women see themselves in a situation of inferiority, and Marxism is again called upon to account for the apparent injustice" (p. 149). For those less than satisfied with such answers the best advice may be (to borrow a phrase of Wittgenstein's) to "take a wider look around" the world that capitalism has made.

THEODORE MILLS NORTON
Vassar College

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Neurosis and Civilization: A Marxist/Freudian Synthesis *by Michael Schneider. New York, The Seabury Press, 1975.—xii, 302 pp. $12.95.*

The basic premise of Michael Schneider's argument is that "the structure of social instincts and needs becomes, with the historical development of the structure of the commodity and money, just as *abstract* as the latter. The 'abstraction' of use values . . . is in a certain sense to be regarded as the germinal political-economic cell of those processes of psychical 'abstraction' which Freud described with his concept of 'instinctual repression'" (p. 122). Abstraction from use-value is inherent in the commodity form; and this "abstraction from the particularity, usefulness, and sensuousness of use value and the particular, useful, and concrete labour at its basis also has as a consequence the abstraction, that is, the tendential 'repression' of all particular, useful, sensuous needs and satisfactions which are bound to the use value" (p. 127). Moreover, both the abstraction of value in the act of exchange and the mental act in which instincts are repressed occur unconsciously, so that consciousness suffers a "loss of reality" with regard to "the sensuous-concrete objectivity of use value and the sensuous-concrete (instinctual) needs that are mediated by them" (p. 127). Such a loss of reality to consciousness is, of course, false consciousness. More specifically, it is what Marx terms the "fetishism of commodities," which Schneider argues arises not only from the commodity form but also from the formation of character in the image of the commodity. The objectively inverted world of "material relations between persons and social relations between things" is thus subjectively reflected in a "perverse displacement of libidinous energies from the world of reified persons onto the world of personified objects" (p. 251).

Schneider generates this conception from a critique of psychoanalysis as bourgeois ideology, and from a parallel critique of the orthodox Marxist attacks upon psychoanalysis; and he utilizes his perspective to analyze the threefold "pathology" of the commodity-money relationship, the capitalist organization of work, and consumerism. In the process he is able to adapt to his own ends virtually all of the most valuable contributions of the Freudian-Marxists (Reich, Marcuse, Fromm, etc.). But he does not really achieve a "Marxist/Freudian synthesis," which would require a synthesizing or unifying principle, a psychoeconomic premise from which to derive theoretical interpretations of empirical material. Schneider's conception of the abstraction from use-value is not such a principle, but rather the sign of a conceptual lacuna. For unlike Marx's concept of the abstraction from or alienation of use-value, which is determined by its relation to human labor power, the idea of repressed "sensuous-concrete (instinctual) needs" is an altogether indefinite expression. It has no particular meaning in itself; and its relationship to Freud's concept of instinct on the one hand and Marx's conception of alienated and unalienated labor on the other is not specified. It follows that the meanings of such critical psychodynamic concepts as repression and sublimation are similarly ambiguous, with the consequence that the nature of consciousness and conscious activity is not determined by but only presented in relation to Schneider's general position.

Finally, it may be observed that, unless one reverts to Hegelianism or is content

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with a purely Kantian eclecticism, no "Marxist/Freudian synthesis" is possible. Either psychoanalysis must be rigorously developed into a Marxist psychology, or Marxist concepts can become metaphors in an extended psychoanalytic mythology. It is to Schneider's credit that (despite the subtitle) he not only undertakes the former project, but brings it substantially closer to realization.

EUGENE VICTOR WOLFENSTEIN
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Ideologies and Illusions: Revolutionary Thought from Herzen to Solzhenitsyn *by Adam B. Ulam. Cambridge, Mass., Harvard University Press, 1976.—vii, 335 pp., $15.00.*

The title of this book is misleading. The volume is actually a collection of essays on topics ranging from Russian intellectual history to domestic Soviet policies, Soviet Communist party congresses, and international affairs. Most of these had appeared over the last decade in various periodicals and anthologies. A few, such as those on Lenin's declining years and on Lenin, Stalin, and Trotsky, have successfully withstood the ravages of age and read as well today as they did when originally published. Indeed, the essays on the founders of the Soviet state will probably continue to be anthologized in years to come: they are among the very best concise treatments of their subjects. By contrast, the article on Titoism is now clearly out of date, a predictable if not unavoidable peril facing political writing on ideologies still in the process of evolution.

An eminent specialist in Soviet politics, Adam B. Ulam is at his best when discussing convolutions of Soviet diplomacy, in particular when Soviet national interests must somehow be reconciled with postulates of Communist faith. Ulam argues most persuasively that the former are invariably decisive. That the latter are not entirely disregarded, that the teachings of communism retain much of their earlier vigor and may, in fact, acquire even more importance in the future, is one of Ulman's central theses. Thus, he points out ironically in his introduction,

> Some years ago—actually only fifteen or so but it seems like ages—many social scientists and other wise men concluded that the era of ideologies was over. The world, whether in Russia or the United States, was increasingly ruled by technocrats. Old faiths and enthusiasms had given way to computers and new scientific techniques guaranteed to solve our social problems and to effect the material and spiritual progress of mankind. . . . The Communist world presented a somewhat less reassuring picture. But after all, Stalinism had been jettisoned, peaceful coexistence had been proclaimed the goal of Soviet policy, and though Khrushchev's rhetoric often clashed with this proclaimed goal, he spoke beguilingly of "goulash Communism," a close approximation of our "consumerism." One could then be cautiously hopeful that a convergence of the two worlds would not bring about a collision (pp. 1–2).

The all-too-transparent allusion to Daniel Bell and some of the more dogmatic

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proponents of the détente is firmly rooted in facts. In fact, Ulam's thesis might be broadened to include continuing conflicts in which communism as such is not a factor. Who would have predicted some decades ago that *religious* wars, such as those between the Christians and the Moslems in Lebanon or between the Catholics and the Protestants in Northern Ireland would claim thousands of lives in the last quarter of the twentieth century?

The least satisfying parts of the book are the opening essays tracing the beginning of Russia's radical ideology ("Bakunin, Herzen and Chernyshevsky" and "Socialism and Utopia") and the closing piece devoted to Alexander Solzhenitsyn. All of them, of necessity, allude to literary works, somehow failing in the process to preserve the delicate balance between artistic and political values. (The appearance of Solzhenitsyn's *Gulag Archipelago* may have been, as Mr. Ulam claims on p. 310, "a historic event," but the book is hardly "a literary masterpiece.") And while both Solzhenitsyn and Herzen represent the spirit of revolt against Russia's ruling ideologies, they have, otherwise, little in common.

MAURICE FRIEDBERG
University of Illinois at Urbana-Champaign

The Conservative Intellectual Movement in America Since 1945 *by George H. Nash. New York, Basic Books, 1976.—xv, 463 pp. $20.00.*

Nash's compendium of postwar conservative intellectuals merits praise and requires criticism. In unpretentious and unemotional prose he charts the evaluation of conservative thought in the United States from 1945 to 1972, from Friedrick Hayek's *The Road to Serfdom* to the recantations of the *Commentary-Public Interest* crowd, ending without reference to Watergate's impact.

Primarily, Nash chronicles three mainstreams of antipathy to modern liberalism and their attempted synthesis: first, the libertarians, led by Hayek, Ludwig von Mises, and Frank Chodorov, who opposed the unchecked expansion of the state as a threat to private enterprise, liberty, and individualism; then the traditionalists, particularly Russell Kirk, Richard Weaver, and Peter Viereck, who urged a return to ethical and religious absolutes; and often drowning out both groups, the host of militant anticommunists, guided by James Burnham, L. Brent Bozell, Whittaker Chambers, and William Rusher, who crusaded against the totalitarian red menace. In the early 1960s, according to Nash, Frank Meyer fused classical liberalism's devotion to laissez-faire, private property, and reason with the authority of God, Truth, and Virtue in the Great Tradition. Fusionism was born, and quickly became the consensus view propounded by the *National Review*. Nash's secondary themes include the search for a viable conservative tradition in American thought, the development of a conservative political philosophy by Leo Strauss and Willmoore Kendall, and the tensions of the 1960s which led to the emergence of conservatism as a powerful intellectual and political force. All this is done in considerable detail and with a high degree of objectivity. As such, it is a contribution of value to our

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knowledge of the past. Ninety pages of notes, an excellent bibliographical essay, and interviews or correspondence with almost 100 prominent conservatives buttress Nash's digest of the major books and essays relevant to his subject.

Critical omissions, however, flaw Nash's work. Virtually nothing is said about conservatism before 1945. Nash's descriptions of who wrote what after the war thus lack historical perspective. He also eschews explanation. Whatever exists is accepted as given. There are no references to either social psychology or cultural anthropology. The perceptive works of Herbert McClosky and Samuel Huntington are barely acknowledged, much less addressed. Worse, Nash never defines his key term. Conservatism sometimes appears to be an attitude, then a faith, or an emotion, even a temper of mind. At other times, Nash writes of it as an epistemology, a commitment, an ideology, a style, a bias against change. Granting the necessity of flexibility in definition, there needs to be some substance to the meaning of conservatism in a work of intellectual history—be it historical, philosophical, or contextual. Without it, virtually anyone or anything can or cannot be labeled conservative; and Nash does just that to suit his purposes, especially in his handling of Viereck and Clinton Rossiter, and in his discussions of those libertarians and traditionalists who rejected fusionism. He also confuses principles with rationalizations. It is unfortunate that Nash ignored John Mitchell's advice: "Don't watch what we say; watch what we do." Too much of the book merely paraphrases conservative intellectuals hailing or belittling one another. Not enough of it elucidates the conservative penchant for superpatriotism or the defense of vested interest. Nash, moreover, is hardly convincing in his demonstration of unity between the diverse emphases on an organic society, a free market, and evangelical anticommunism. Such issues as ending the draft, censorship, abortion, capital punishment, support for right-wing dictatorships, the decriminalization of certain drugs, and détente have underscored the lack of agreement among Nash's conservatives. Finally, we are told that conservatism is winning, but not what that means. No commonality of goal shared by the writers of the *National Review* and the voters of Middle America is ever demonstrated. Indeed, Nash's own evidence of success applies mainly to the right-wing of American liberalism. Nash is currently preparing a multivolume biography of Herbert Hoover—who persisted in describing himself as a liberal. Before he publishes again Nash should read George Orwell's essay on language and politics.

HARVARD SITKOFF
University of New Hampshire

Norman Thomas: The Last Idealist *by W. A. Swanberg. New York, Charles Scribner's Sons, 1976.—xii, 528 pp. $14.95.*

The life and career of Norman Thomas illustrates the trials and tribulations of the moralist who wishes to be a political man as well. What made Thomas so towering a figure for over fifty years of American political history was the fact that, though

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he acted in terms of deep-seated moral convictions rooted in a secularized version of Christian moral perfectionism, he never lost touch with the American tradition of pragmatic give-and-take. His life bears testimony to the fact that Weber's famous distinction between the ethics of conviction and the ethics of responsibility has only limited utility in that they are not mutually exclusive. Weber distinguished between political actions guided by the purity of convictions which are unconcerned with the weighing of means and consequences, and political activities that assess the consequences of actions in terms of their possible impact on other actors and the relations between ends and means. For Thomas there was no such distinction. He steadily held to a vision of human brotherhood when most of his contemporaries were content with striving for incremental gains, yet he never succumbed to the temptation to remove himself from the heat of political contentions by playing the Thoreauian role of the purist above the battle. He eschewed the characteristic defects of so many American reformers and radicals in that he became neither a tinkerer nor a crank.

W. A. Swanberg's ample biography, almost Victorian in its design and breadth, provides the necessary ingredients for an assessment of Thomas' role and his impact on his times while yet not offering such an assessment. Swanberg has interviewed almost all of Thomas' surviving associates. He has gone assiduously through Thomas' papers now deposited in the New York Public Library, and he has profited from a number of previous studies. As a result he has been able to paint a full-scale portrait of the man and a record of his multifaceted involvements with the immense variety of public issues that marked his career. Yet the author fails to account for the fact that Thomas, while magnificently successful in the sponsoring of many causes that helped improve and enhance the American political scene, yet also lived to see the practical demise of the Socialist party to which he had devoted most of his ample energies.

We have to wait for more analytical works to help us assess what in this failure was due to objective circumstances such as the rise of the New Deal, and what can, perhaps, be accounted for by Thomas' personal failings. For the time being we are left with the paradox that this great circuit rider for socialism converted few to its cause, while he yet served as a beacon of moral rectitude for many more whose lives were enriched and whose conscience was invigorated by his unflagging devotion to the politics of morality. What Georg Simmel once wrote about his own work would also seem to apply to Thomas': his legacy came, as it were, in cash, distributed to many heirs, each transforming his part into use conformed to *his* nature—a use which revealed no longer its indebtedness to this heritage. Even though he suffered defeat in his central quest, he immensely enriched the quality of American political life.

Let the last word be Max Weber's. That supreme political realist, transcending his own conceptual distinctions quoted earlier, wrote toward the end of his life, "All historical experience confirms the truth that mankind would not have attained the possible unless time and again it had reached out for the impossible." Steadily

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striving for the "impossible," Thomas helped enlarge the politics of the possible. And we all are the richer for it.

LEWIS A. COSER
State University of New York at Stony Brook

Current Research in Comparative Communism: An Analysis and Bibliographic Guide to the Soviet System *by Lawrence L. Whetten. New York, Praeger Publishers, 1976.—vii, 159 pp. $16.50.*

The Democratic-Republican Societies, 1790–1800: A Documentary Sourcebook of Constitutions, Declarations, Addresses, Resolutions, and Toasts *edited by Philip S. Foner. Westport, Conn., Greenwood Press, 1977.—xiii, 484 pp. $29.95.*

Foreign Relations of the United States 1950, Vol. II: The United Nations; The Western Hemisphere *prepared by Department of State. Washington, D. C., Government Printing Office, 1976.—xii, 1088 pp. $13.00.*

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Correspondence

More on Soviet Control of Eastern Europe

To the Editor:

Roy E. Licklider in the Winter 1976–77 issue advocated tight Soviet control over Eastern Europe in order to prevent a nuclear war. His arguments were based on poorly understood geopolitical history of the region and a willingness to forgo moral obligations in American foreign policy toward Eastern Europe.

Few would believe and be willing to endorse oppressive domination of people anywhere in order to maintain unity and to prevent disorder, particularly when these problems can be solved with goodwill and a sense of responsibility. Above all, the United States must depend on oppression by another major power, just to be free from the threat of war—conventional or nuclear.

There is no need for tightfisted Soviet policy in Eastern Europe to prevent a nuclear confrontation because there is a solution available which could serve the national interest of the United States, the interest of Europe, and of mankind.

In the so-called Philadelphia Declaration of 1951, the free-world representatives of Albanians, Bulgarians, Czechoslovakians, Estonians, Lithuanians, Hungarians, Poles, Rumanians, and Yugoslavians declared for an East-Central European Federation, as soon as Soviet domination is lifted.

This plan was published in a "Blueprint" in the *Studies for a New Central Europe* (Series 3, no. 1, 1971–72). It would create a new zone of federated states under the guarantee of the United Nations and the Great Powers represented in its Security Council. Severe military limitations would be imposed and maintained with independence and sovereignty of the states. Courts of arbitration would deal with various aspects, and the rights of minorities would be based on the human rights of the United Nations.

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This morally acceptable solution appears to be more in the national interest of the United States and the Soviet Union than the enforced status quo.

DR. CORNELIUS NAVORI
Grosse Ile, Mich.

To the Editor:

It was to be expected that a provocative, though objective, article such as Licklider's, in your Winter issue, would arouse a "knee-jerk" reaction from others, but was it really necessary to inspire such a hasty and puerile response? As Licklider makes clear in the first paragraph of his "Rejoinder," the first two and one-half pages of "Ploughman's" commentary are waste paper, being directed at a target having nothing to do with the original proposition.

Likewise, "Ploughman" refrains from addressing himself to the possible (though unlikely) scenario in which "a future Soviet government may be tempted to pull out of the area [of Central Europe]." It would be instructive to consider this theoretical scenario in trying to reach the heart of the author's theme. But "Ploughman" is unable to do this because his whole argument is based on an appeal to emotion as demonstrated by his use of such terms as "an aggressive big power" and "appeasement or surrender." The "one world" approach he ends up advocating is a "pie-in-the-sky" evasion of the dilemma facing the United States not only in Europe, where for the past thirty years our rhetorical policy has had a free ride, but also in the Middle East. Since "Ploughman" chooses the loaded term "Yom Kippur War" instead of the objective term "October War," I imagine he has preconceived notions about the Middle East, as well.

There must be some good arguments against Mr. Licklider's hypothesis, but they have yet to be adduced.

ROLLAND BUSHNER
New York, N. Y.

Professor Licklider replies:

An Eastern European federation as suggested by Dr. Navori would solve most of the problems mentioned in my article and bring American national interests into line with morality. However, I believe that such a development is unlikely to occur. While a common enmity toward the Soviet Union may foster such unity, it seems unlikely to persist in the face of the social and political centrifugal forces outlined in the article. Experiences in areas as diverse as Western Europe, postcolonial

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states in the Third World, and Eastern Europe itself suggest that the imposition of a formal, legal structure of unification will not prevent fragmentation.

ROY E. LICKLIDER
Douglass College, Rutgers University

Piers Ploughman replies:

Like Rolland Bushner, I found Roy Licklider's article about Soviet control of Eastern Europe to be provocative; indeed it provoked me into attempting a gentle rebuttal. Hopefully, this "knee-jerk reaction" has remained within the bounds of civilized discourse.

But I bridle at the suggestion that I ignored Licklider's scenario about the destabilizing effects of a Soviet pullout from Eastern Europe. The fact is, I am in Licklider's corner on that score: a Soviet withdrawal is liable to cause problems, and the East Europeans do have a propensity to be exasperatingly quarrelsome. But then there are serious problems and quarrels aplenty in other areas, including Africa, Asia, and even Western Europe, which does not necessarily justify handing them over to the Soviet Union, or to any other would-be hegemonistic power, in an effort to purchase a bit of peace and quiet. Once we embrace the theory that the East Europeans must remain, *nolens volens*, under Soviet military and political control, what basis is there left for objecting to the same kind of relationship elsewhere?

Of course Soviet control of Eastern Europe is likely to continue for the foreseeable future, as Licklider points out in his thoughtful "Rejoinder to Piers." Nobody in his right mind is proposing to march American soldiers into Czechoslovakia. But surely we do not have to cheer the continued, and clearly oppressive, presence of Soviet soldiers in that country, or elsewhere in the region. Nor should we seek arrogantly, in collusion with the Soviet Union, to deny freedom and self-determination to the East Europeans or other "lesser breeds without the law" on the grounds that their corner of the globe is somehow "distinctive."

Let us continue, firmly but peacefully, to advocate a relaxation of Soviet control over Eastern Europe, an end to the remnants of the cold war (on both sides, to be sure), and decent respect for fundamental human rights. If that is what the new administration is trying to achieve, it deserves our support.

PIERS PLOUGHMAN

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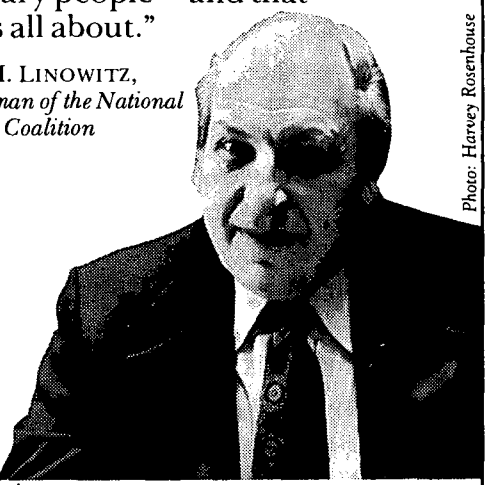

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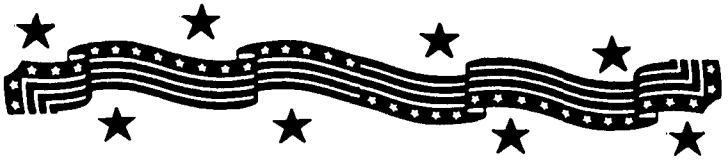

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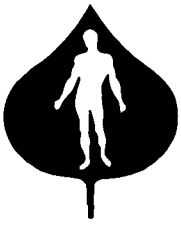

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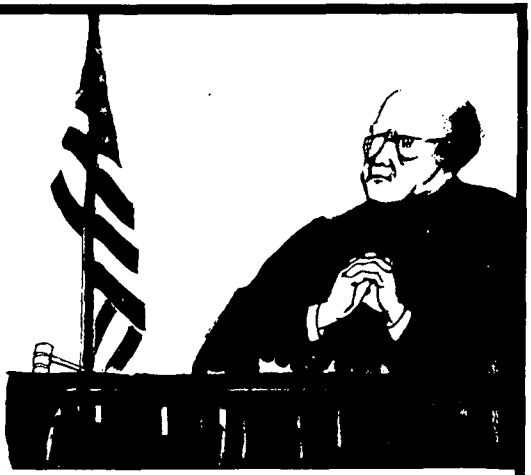

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